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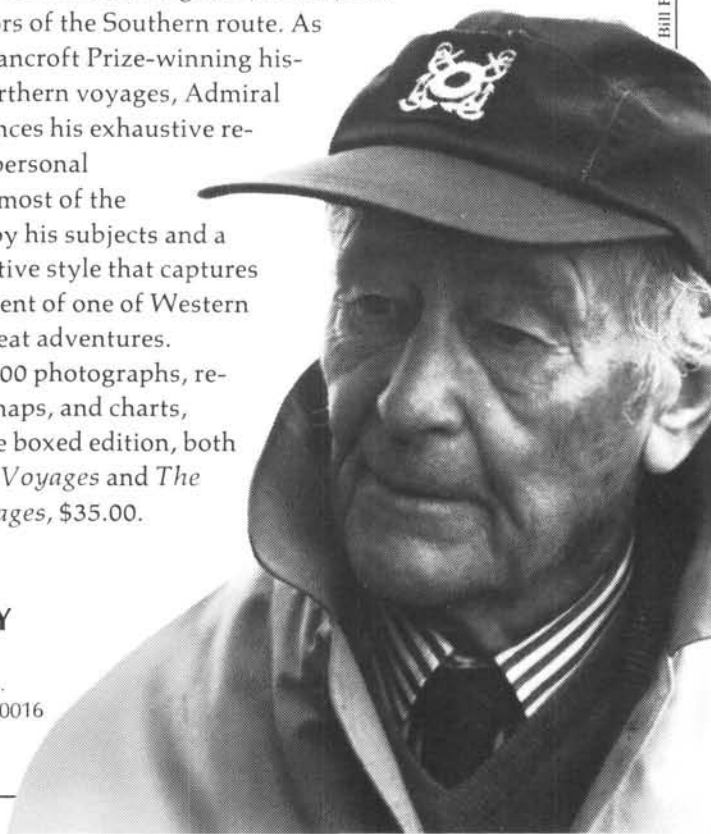
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VOLUME 79 • NUMBER 4 • OCTOBER 1974

The American Historical Review

THE AMERICAN HISTORICAL ASSOCIATION

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The American Historical Review appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003, and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228.

The *AHR* is sent to members of the American Historical Association and to institutions holding subscriptions. Membership dues: \$20.00 annually; student (faculty signature required), emeritus and spouse \$10.00; full professor and nonacademic \$25.00; life \$400. Subscription rates: Class I, *American Historical Review* only, United States, Canada, and Mexico \$25.00, foreign \$27.00. Further information concerning membership and subscriptions is contained in the two pages immediately preceding the advertisements. Information concerning the ordering of back issues and the submission of manuscripts will be found on the page immediately preceding the advertisements.

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The Tyranny of a Construct: Feudalism and Historians of Medieval Europe

ELIZABETH A. R. BROWN

AT A RECENT CONFERENCE Thomas N. Bisson introduced his paper "Institutional Structures of the Medieval Peace" by cautioning his audience that in his discussion of peace movements, peace associations, and peace institutions in southern France and Spain he would not attempt to relate his findings to "feudalism."¹ His approach was descriptive—and thoroughly enlightening—and no further reference to any ism occurred until the question period. Then, bestowing the double-edged praise that is his hallmark, Professor John F. Benton asked how historians could have managed to overlook for so long such abundant evidence that would necessitate the revision of numerous lectures on medieval society. Responding to this remark, Professor Bisson again alluded to the eventual necessity of evaluating his conclusions with reference to the general topic of feudalism, but time prevented him from elaborating. It occurred to me as this interchange was taking place that the failure of historians to take account of the data used by Bisson may well have resulted from their concentration on feudalism—as model or Ideal Type—and their consequent tendency to disregard or dismiss documents not easily assimilable into that frame of reference.

Whatever their relevance to the subject of Professor Bisson's paper, feelings of uneasiness concerning the term "feudalism" are not uniquely mine. Historians have for years harbored doubts about the term "feudalism" and the phrase "feudal system," which has often been used as a synonym for it. One of the first, and certainly one of the wittiest and most eloquent,

An earlier version of this article was presented to a meeting of the Columbia University Seminar on Medieval Studies, May 8, 1973. I am grateful to the members of the seminar for their questions and suggestions. For their advice and counsel I would also like to express my thanks to Professor Fredric Cheyette of Amherst College, Professor John Bell Henneman of the University of Iowa, Professor Joshua Prawer of the Israel Academy of Sciences and Humanities, Professor Thomas N. Bisson of the University of California at Berkeley, Professor John F. Benton of the California Institute of Technology, Professors Edwin Burrows, Philip Dawson, Charlton Lewis, and Hyman Sardy of Brooklyn College of the City University of New York, Barbara W. Tuchman, and finally the members of the History Club and my students at Brooklyn College.

¹ Thomas N. Bisson, "Institutional Structures of the Medieval Peace," a paper presented to a colloquium held at Princeton University on March 31, 1973.

to comment on the problem was Frederic William Maitland. In lectures on English constitutional history prepared in 1887 and 1888 he wrote:

Now were an examiner to ask who introduced the feudal system into England? one very good answer, if properly explained, would be Henry Spelman, and if there followed the question, what was the feudal system? a good answer to that would be, an early essay in comparative jurisprudence. . . . If my examiner went on with his questions and asked me, when did the feudal system attain its most perfect development? I should answer, about the middle of the last century.²

Thanks to J. G. A. Pocock, it is now known that Henry Spelman, a learned English antiquarian of the seventeenth century, used neither the term "feudal system" nor the word "feudalism," but this does not detract from the validity or the importance of Maitland's observations. Following in the steps of the Scottish legal scholar Sir Thomas Craig, Spelman held that the social and political relationships of medieval England had been uniform and systematic enough to be described adequately as regulated by a "'feudal law' [which] was an hierarchical system imposed from above as a matter of state policy." The work of Craig and Spelman had its virtues, for they were the first British historians to attempt to relate British institutions to continental developments. Both, however, relied for their knowledge of continental institutions on Cujas's and Hotman's sixteenth-century editions of the twelfth-century Lombard *Libri Feudorum*, which gave, to paraphrase Pocock, a precise and detailed "definition of the *feudum* whereby it could be recognized in any part of Europe," or, as he says, "a systematic exposition of the principles of tenure, forfeiture and inheritance." These criteria Craig and Spelman employed to classify the evidence from Scottish and English sources, and their simplification and regimentation of phenomena notably offset the advantages to historical thought of their demonstration that the development of England and Scotland could be understood only in the context of the European experience.³

Given these beginnings, it is no wonder that eighteenth-century British writers began to accept the concept of a uniform feudal government and to concentrate on the system, the construct, instead of investigating the various social and political relationships found in medieval Europe. "They were," Pocock observes, "making an 'ism' of [feudalism]; they were reflecting on its essence and nature and endeavoring to fit it into a pattern of general ideas."⁴ In so doing they resembled Boulainvilliers and Montesquieu, who

² Frederic William Maitland, *The Constitutional History of England*, ed. H. A. L. Fisher (Cambridge, 1908), 142. See also Fisher's introduction to this edition, p. v.

³ J. G. A. Pocock, *The Ancient Constitution and the Feudal Law: English Historical Thought in the Seventeenth Century* (Cambridge, 1957), 70 n.2, 93-94, 249, 79-80, 97-99, 70-79, 72, 84, 99, 103, 102. Pocock perhaps exaggerates these advantages (p. 102) because of the strength of his admiration for the boldness and imagination with which Craig and Spelman challenged the distortedly insular approach taken by Coke and the common lawyers. It seems clear, furthermore, that Pocock himself does not question the validity or the usefulness of the term "feudalism."

⁴ *Ibid.*, 249; see also Robert Boutruche, *Seigneurie et féodalité: Le premier âge des liens d'homme à homme* (Paris, 1959), 15 nn.16-17, 16 n.20.

wrote of *féodalité* and *lois féodales* as distinguishing a state of society, thus, incidentally, expanding the concept to include a far wider range of phenomena than it had for legal scholars.⁵ The writers of the eighteenth century, like those of later times, assigned different meanings to the term *féodalité*, or, in English, “feodality.” Some used it to designate a system of government, some to refer to conditions that developed as public power disappeared. By 1800 the construct had been launched and the expression “feudal system” devised; by the mid-nineteenth century the word “feudalism” was in use. The way was prepared for future scholars to study feudalism—whatever it was conceived to be—scientifically and for others to employ the ism to refer, abusively, to those selected elements of the past that were to be overthrown, abolished, or inexorably superseded.⁶

SINCE THE MIDDLE OF THE NINETEENTH CENTURY the concepts of feudalism and the feudal system have dominated the study of the medieval past. The appeal of these words, which provide a short, easy means of referring to the European social and political situation over an enormous stretch of time, has proved virtually impossible to resist, for they pander to the human desire to grasp—or to think one is grasping—a subject known or suspected to be complex by applying to it a simple label simplistically defined. The great authority of these terms has radically influenced the way in which the history of the Middle Ages has been conceptualized and investigated, encouraging concentration on oversimplified models that are applied as standards and stimulating investigation of similarities and differences, norms and deviations. As a result scholars have disregarded or paid insufficient attention to recalcitrant data that their models do not prepare them to expect.

But let us return to Maitland. Implicit in his assessment of Spelman and the feudal system is a clear objection to applying the label “feudal system” to medieval England, presumably because of a belief that England never underwent a systematization of social and political life—or, as Maitland puts it, never experienced “the development of what can properly be called a feudal system.” Less evident, perhaps, is a hesitancy about the propriety of using the phrase “feudal system” at all. That Maitland questioned the wisdom of applying it to conditions of medieval society is hard to dispute, however, for in his lectures he remarks,

The phrase [feudal system] has thus become for us so large and vague that it is quite possible to maintain that of all countries England was the most, or for the

⁵ Boutruche, *Seigneurie et féodalité*, 13–14; Marc Bloch, *La Société féodale* (Paris, 1949), 1: 1–3. The English edition, with a foreword by M. M. Postan, was translated by L. A. Manyon and is entitled *Feudal Society* (Chicago, 1961); the corresponding pages are xvi–xviii.

⁶ Boutruche, *Seigneurie et féodalité*, 16, 18–23. See also the *Oxford English Dictionary*, s.v. “feudal,” “feudalism,” and “feudality.”

matter of that the least, feudalized; that William the Conqueror introduced, or for the matter of that suppressed, the feudal system.⁷

Still, having bemoaned the terminological situation, Maitland proceeds to use the term "feudalism," equated by him with "feudal system."⁸ He announces that "the feudalism of France differs radically from the feudalism of England, that the feudalism of the thirteenth is very different from that of the eleventh century." He then goes on to give his own definition of feudalism, emphasizing ties of vassalage, fiefs, service in arms owed the lord, and private administration of justice. Using this definition, he discusses the question of the progress toward such an organization that England had been making before the Norman Conquest, and he concludes, "Speaking generally then, that ideal feudalism of which we have spoken, an ideal which was pretty completely realized in France during the tenth, eleventh and twelfth centuries, was never realized in England." Here, he says, "the force of feudalism [was] limited and checked by other ideas."⁹

As these statements show, Maitland's tolerance for unresolved contradictions was high, and other historians have demonstrated a similarly striking capacity for living with inconsistency. Although they attack the term "feudalism," they are still unwilling and perhaps unable—whether from habit, inertia, or simple inattention—to jettison the word. Consider H. G. Richardson and G. O. Sayles. In a book published in 1963 they denounce "feudal" and "feudalism" as "the most regrettable coinages ever put into circulation to debase the language of historians." "We would, if we could," they declare, "avoid using them, for they have been given so many and such imprecise meanings." They confess, however—without apology or explanation—that they cannot "rid [themselves] of the words and must live with them" and therefore proclaim their determination to "endeavor, when [they use] them, to do so without ambiguity." They evidently have some sense of attachment, however grudging, to the terms, and their feelings are reflected in their insistence that "if the concept and the term are to be in the least useful"—thus implying that they can be—"there must be precise definition." Such definition they do not, unfortunately, offer. Nonetheless they doggedly persist in using the words, and they spend a large portion of their book dealing with their "thesis of the relative unimportance of any

⁷ Maitland, *Constitutional History*, 161, 143. See also Sir Frederick Pollock and Frederic William Maitland, *The History of English Law before the Time of Edward I* (2d ed., introd. S. F. C. Milsom; Cambridge, 1968), 1: 66-67; and Frederic William Maitland, *Collected Papers*, ed. H. A. L. Fisher (Cambridge, 1911), 1: 489.

⁸ Maitland does not subject the word "feudalism" to the same critical scrutiny he applies to the phrase "feudal system," and he is far less wary of using the former than the latter. At one point in his lectures he seems to be distinguishing between the two—"we do not hear of a feudal system until long after feudalism has ceased to exist"—but he also uses them as equivalents. In his conclusion he indicates that he considers "the development of . . . a feudal system" the same as the realization of "ideal feudalism." *Constitutional History*, 141-43, 161-63.

⁹ *Ibid.*, 143-64.

element of 'feudalism' in post-Conquest England" and of "the essential continuity of English institutions."¹⁰

Such an approach logically requires isolating those elements that can properly be called feudal from those that cannot. Since Richardson and Sayles never explicitly objectify the enemy, however, their readers are left to deduce from their arguments just what phenomena they consider essential components of feudalism. Homage, " 'feudal' incidents," honors and honorial courts, knightly service connected with fiefs, and the use of military tenures for military purposes are all linked in one way or another with feudalism, although Richardson and Sayles clearly suggest that, unless found in their Franco-Norman forms, these elements should not be considered truly feudal. Thus the authors attempt to validate their hypothesis by showing either that these or similar institutions existed in England before 1066—and hence are to be classified as Old English and therefore not Norman feudal—or that they had no real importance after that date.¹¹ In the end, coming to grips with the problem of definition, they abruptly abandon their previous criteria. So that they can pronounce England safely nonfeudal and therefore non-French, they fall back on what they call "the classical theory of feudalism," described as the idea of lordship diminished by fragmentation or of "sovereignty . . . divided between the king and his feudataries," neither of which was ever found in England. They warn that feudalism should not be defined simply in terms of tenure, since if it is it will be found everywhere.¹²

As their lengthy discussion and conclusion make clear, Richardson and Sayles were never fully convinced, despite their initial volleys, that feudalism was in fact no "more than an arbitrary pattern imposed by modern writers upon men long dead and events long past." Although they end their analysis by remarking of the word "feudal" that "an adjective so ambiguous and so misleading is best avoided," their repeated use of the term belies their alleged distaste.¹³

IF NUMEROUS ARGUMENTS in defense of feudalism have been advanced, "utility" and "indispensability" are the chief rallying cries of the term's defenders. Let us turn first to the criterion of utility.

In the introduction to his classic study *Feudalism*, F. L. Ganshof states that he intends his book to facilitate the work of students of medieval society. In analyzing and describing feudal institutions he says he has "endeavored to bring out as clearly as possible their essential features, since, once these

¹⁰ H. G. Richardson and G. O. Sayles, *The Governance of Mediaeval England from the Conquest to Magna Carta* (Edinburgh, 1963), 30, 92, 117–18, 30–31, 105, 116.

¹¹ *Ibid.*, 36–38, 77, 99, 105–12, 115; see also 85–91, 147, and the comments on p. 116: "The Normans were already familiar with much that they found in England, but we are not thereby warranted in terming those familiar things 'feudal' or in asserting that England was already 'feudal.'"

¹² *Ibid.*, 117–18.

¹³ *Ibid.*, 92, 118.

are grasped, it is easy for the student to disentangle the elements that can properly be described as feudal in the institutions of the period or country with which he is primarily concerned."¹⁴ Helping the scholar as well as the student to evaluate, analyze, and categorize the past is also important to Michael Postan, and in his foreword to the English edition of Marc Bloch's *Feudal Society* he argues that the usefulness of "generalized concepts" such as feudalism lies in their ability to "help us to distinguish one historical situation from another and to align similar situations in different countries and even in different periods." For Postan greater complexity apparently means greater utility, and he prefers Bloch's definition of feudalism, which embraces "most of the significant features of medieval society," to "constitutional and legal concepts of feudalism" centering on "military service" and "contractual principles." These latter concepts, he feels, may have some virtue as pedagogical devices, to promote "intellectual discipline," and to serve as "an antidote to the journalistic levities of modern historiography." Still, they cannot validly be considered "an intellectual tool, to be used in the study of society."¹⁵

If Postan draws a rather unsettling distinction between pedagogy on the one hand and research and sound intellectual endeavor on the other, it is clear that he is not alone in considering appropriate for the student what is decried for the scholar. This "track" approach to feudalism is widespread, even though those who espouse it may differ concerning what should be taught at different levels. Postan envisions progression from a partial to a more complex model, always retaining the term "feudalism" to denote the model. Others, expressing fundamental objections to the misleading impression of simplicity and system they believe inevitably associated with isms, still argue that authors of basic textbooks—as opposed to advanced studies—would be lost without the concept of feudalism. This rather inconsistent attitude apparently springs from two convictions: first, that beginning students are incapable of dealing with complex and diverse development and must for their own good be presented with an artificially regular schema; and second, that the term "feudalism" somehow helps these students by serving as a handy, familiar tag to which to attach consciously oversimplified generalizations. Later, as graduate students, they are presumably to be introduced to qualifications and complications, and finally, as scholars and initiates into the mysteries of the trade, they are to be encouraged to discard the offending ism for purposes of research, if not for purposes of teaching their own beginning students. Charles T. Wood, although not explicitly endorsing the use of the term "feudalism," writes that "the feudal pyramid . . . makes for clear diagrams, and schoolboys have to begin somewhere."

¹⁴ F. L. Ganshof, *Feudalism*, foreword F. M. Stenton, tr. Philip Grierson (London, 1952), xviii; see also 151.

¹⁵ Postan, foreword to Bloch, *Feudal Society*, xiv, xiii.

Still, he admits, "where they do begin is rather far removed from reality."¹⁶

Postan, and presumably Ganshof, feels that employing the construct has the virtue of enabling scholars to distinguish likenesses among different times and areas. Similarly John Le Patourel advocates formulating a definition of feudalism that could be used "as a measuring-rod,"¹⁷ and such a standard could presumably be relied on not only, as he wants, to clarify "the old argument" over the introduction of feudalism into England but also, as Postan argues, to advance the work of those concerned with comparing developments in different countries.

If feudalism is praised as a teaching device and as a means of understanding societies, it is also said to be "indispensable," and that for a number of reasons. Marc Bloch maintains that scientists cannot function without abstractions and that since historians are scientists, they also require abstractions. The specific abstractions "feudal" and "feudalism" are defended on the grounds that, however awkward and inappropriate in terms of their original connotations these words and others like them may be, the historian is in this respect no worse off than the scientist, who must also make do with inconvenient and unsuitable terminology.¹⁸ Michael Postan goes beyond Bloch to declare that "without generalized terms representing entire groups of phenomena not only history but all intelligent discourse would be impossible," and he maintains that no difference exists between such a word as "feudalism" and other general terms like "war" and "agriculture."¹⁹ Equally positively, if less aggressively, Fredric Cheyette has insisted that the term "feudalism" cannot "simply be discarded—the verbal detours one would have to make to replace it would be strained as well as disingenuous."²⁰ Otto Hintze argues that the concept is indispensable not only for reasons of practicality and convenience but also because of the deficiencies of the processes of human thought, assumed to be incapable of comprehending the complexities of the real world. Hintze asserts that since "it is impossible to grasp the complicated circumstances of historical life, so laden with unique occurrences, in a few universal and unambiguous concepts—as is

¹⁶ Wood's own description of medieval society deals with human beings rather than schemas, but he occasionally uses the terms "feudal" and "feudalism," which are not defined. *The Quest for Eternity: Medieval Manners and Morals* (New York, 1971), 28, 55–56, 177. Wood's index (p. 227) shows that he has not discarded the term "feudalism," which he seems to see as closely linked with vassalage.

¹⁷ John Le Patourel, review of Richardson and Sayles, *Governance of Mediaeval England*, in *English Historical Review*, 80 (1965): 117 n.1; and see also Max Weber, *The Theory of Social and Economic Organization*, ed. Talcott Parsons, tr. A. M. Henderson and Talcott Parsons (New York, 1947), 329.

¹⁸ Marc Bloch, *Apologie pour l'histoire ou Métier d'historien* (Paris, 1949), 86–87. The corresponding pages in the English edition—*The Historian's Craft*, ed. Lucien Febvre, tr. Peter Putnam (New York, 1953)—are 169–71.

¹⁹ Postan, foreword to Bloch, *Feudal Society*, xiv.

²⁰ Fredric L. Cheyette, "Some Notations on Mr. Hollister's 'Irony,'" *Journal of British Studies*, 5 (1965): 4; see also Cheyette, ed., *Lordship and Community in Medieval Europe: Selected Readings* (New York, 1968), 2–3.

done in the natural sciences," historians must use "intuitive abstractions" and create "Ideal Types, and such types indeed underlie our scholarly terminology."²¹

Even its most eloquent advocates readily acknowledge the difficulties associated with the use of the term "feudalism." Marc Bloch, for one, states that "nearly every historian understands the word as he pleases," and "even if we do define, it is usually every man for himself." He admits that the word is charged with emotional overtones²² and is in fact "very ill-chosen,"²³ and he acknowledges that, in general, abstractions which are "ill-chosen or too mechanically applied" should be avoided.²⁴ He goes so far as to declare that the word "capitalism" has lost its usefulness because it has become burdened with ambiguities and because it is "carelessly applied to the most diverse civilizations," so that, as a result, "it almost inevitably results in concealing their original features."²⁵ Even Postan, whose loyalty to Bloch exceeds Bloch's sense of commitment to his own ideas, grants that comprehensive terms like "feudalism" "over-simplify the reality they purport to epitomize," and he confesses that

in some contexts the practice of giving general names to whole epochs can even be dangerous, [luring] its practitioners into the worst pitfalls of the nominalist fallacy, and [encouraging] them to endow their terms with real existence, to derive features of an epoch from the etymology of the word used to describe it or to construct edifices of historical argument out of mere semantic conceits.²⁶

THE VARIETY OF EXISTING DEFINITIONS of the term and the general unwillingness of any historian to accept any other historian's characterization of feudalism constitute a prime source of confusion. The best definition would doubtless be, as Cheyette suggests, one that helped "to make the body of evidence on medieval institutions coherent," but he himself has not found or formulated any definition to accomplish this purpose.²⁷ In the absence of consensus, the play with meanings has flourished and still continues.

²¹ Otto Hintze, "Wesen und Verbreitung des Feudalismus" (1929), in Hintze, *Gesammelte Abhandlungen*, ed. Gerhard Oestreich, 1 (2d ed.; Göttingen, 1962): 85; for an English translation of the article, entitled "The Nature of Feudalism," see Cheyette, *Lordship and Community*, 22-31. See, too, the comments of Michael Lane and particularly the enlightening passage quoted from Max Weber, in which Weber describes how ideal types are formulated. *Introduction to Structuralism* (New York, 1970), 25-26.

²² Bloch, *Apologie*, 89, 87 (*Historian's Craft*, 176, 171).

²³ "Un mot fort mal choisi." Bloch, *Société féodale*, 1: 3 (*Feudal Society*, xviii).

²⁴ Bloch, *Apologie*, 88 (*Historian's Craft*, 173). Bloch comments that the feudalisms which scholars have located in different parts of the world "bear scarcely any resemblance to each other." *Apologie*, 89 (*Historian's Craft*, 175-76).

²⁵ *Ibid.*, 88 (*Historian's Craft*, 174). For a fuller, if less extreme, analysis of the similar problems posed by using the terms capitalism and feudalism, see the review of J. Q. C. Mackrell, *The Attack on 'Feudalism' in 18th Century France* (London, 1973), in the *Times Literary Supplement*, Feb. 15, 1974, p. 160.

²⁶ Postan, foreword to Bloch, *Feudal Society*, xiv.

²⁷ Cheyette, "Some Notations on Mr. Hollister's 'Irony,'" 4, 12; see also 5-6, where he states that the usefulness of the term (he may in fact mean of the definition) "is determined by how it helps to order the evidence."

The sweeping perspective adopted by Marc Bloch produced a definition of European feudalism—equated by Bloch with feudal society and, in the translation of his book, with feudal system²⁸—that in effect summarizes the topics treated in the central section of his *La Société féodale*. It encompasses a wide range of aspects of medieval life:

A subject peasantry; widespread use of the service tenement (i.e. the fief) instead of a salary, which was out of the question; the supremacy of a class of specialized warriors; ties of obedience and protection which bind man to man and, within the warrior class, assume the distinctive form called vassalage; fragmentation of authority—leading inevitably to disorder; and, in the midst of all this, the survival of other forms of association, family and State, of which the latter, during the second feudal age, was to acquire renewed strength.²⁹

Some historians have accepted this inclusive list as a definition of feudalism, but others would prefer to link it only with feudal society, which they feel can and should be distinguished from a more narrowly conceived feudalism, in which the fief is accorded greater prominence than Bloch gives it.³⁰ Ganshof, for one, believes that in the Middle Ages “the fief, if not the cornerstone, was at least the most important element in the graded system of rights over land which this type of society involved.” The definition of feudalism he prefers—“the narrow, technical, legal sense of the word”—concentrates on service and maintenance and emphasizes the fief, while it excludes entirely the private exercise of public justice and jurisdiction. For Ganshof feudalism is envisaged as

a body of institutions creating and regulating the obligations of obedience and service—mainly military service—on the part of a free man (the vassal) towards another free man (the lord), and the obligations of protection and maintenance on the part of the lord with regard to his vassal. The obligation of maintenance had usually as one of its effects the grant by the lord to his vassal of a unit of real property known as a fief.

²⁸ Bloch, *Société féodale*, 2: 244–49 (*Feudal Society*, 443–45). In the translation (p. 443) “the feudal system” replaces Bloch’s “le régime féodal” (2: 245). Similarly, Bloch’s “les féodalités d’importation” (1: 289–92) become in translation “the imported feudal systems” (pp. 187–89). Both Ganshof and David Herlihy have indicated—misleadingly it seems to me—that Bloch perceived a fundamental difference between feudalism and feudal society. Ganshof, *Feudalism*, xvi; David Herlihy, ed., *The History of Feudalism* (New York, 1970), xix.

²⁹ Bloch, *Société féodale*, 249–50 (*Feudal Society*, 446).

³⁰ Herlihy, *History of Feudalism*, xix; Ganshof, *Feudalism*, xv. Ganshof’s description of feudalism as a form of society on the same page diverges at many points from Bloch’s: “a development pushed to extremes of the element of personal dependence in society, with a specialized military class occupying the higher levels in the social scale; an extreme subdivision of the rights of real property; a graded system of rights over land created by this subdivision and corresponding in broad outline to the grades of personal dependence just referred to; and a dispersal of political authority amongst a hierarchy of persons who exercise in their own interest powers normally attributed to the State and which are often, in fact, derived from its break-up.” Here there is no mention of peasantry or family; here the state is mentioned only by virtue of its dissolution (although see also pp. 141–51 for a lengthy discussion of feudalism and the state); here there is a stress on landed rights and property missing in Bloch’s definition.

Although Ganshof admits that "powers of jurisdiction [in particular what one normally calls feudal jurisdiction] were . . . very closely bound up with feudal relationships," he states firmly that "there was nothing in the relationships of feudalism . . . which required that a vassal receiving investiture of a fief should necessarily have the profits of jurisdiction within it, nor even that he should exercise such jurisdiction."³¹

Ganshof may have his followers, particularly among historians of the Normans and the English.³² On the other hand, many scholars insist that the private exercise of public governmental authority—an element rejected by Ganshof—is the single essential component in any definition of feudalism. Several years ago Joseph R. Strayer adopted this position when he advocated a definition focusing on jurisdiction and omitting most of the other factors contained in the definitions just examined. "To obtain a usable concept of feudalism," Strayer argued, "we must eliminate extraneous factors and aspects which are common to many types of society." Having lopped off aristocracy, "the great estate worked by dependent or servile labor," "the relationship between lord and man," and "the system of dependent land tenures," he concluded that it is "only when rights of government (not mere political influence) are attached to lordship and fiefs that we can speak of fully developed feudalism in Western Europe."³³ Subsequently Strayer

³¹ Ganshof, *Feudalism*, xvi-xvii, 143, 141.

³² Similar to but narrower than Ganshof's is the definition of feudalism offered by D. C. Douglas. Since Douglas's works deal primarily with Normandy and the Norman conquests it is understandable that, like Ganshof, he should not consider the disintegration of central control a basic element. For Douglas two ideas are important: "the principle that the amount of service owed should be clearly determined before the grant of the fief" and "the notion of liege-homage." *The Norman Achievement, 1050-1100* (Berkeley, 1969), 177; see also 179. Douglas also emphasizes the idea of contractual military service, isolating this as the core of the "Norman feudal custom," which, he says, William the Conqueror interpreted "in a sense advantageous to himself" when he "[suddenly introduced] military feudalism into England." *William the Conqueror: The Norman Impact upon England* (Berkeley, 1964), 100, 101, 103, 283. See also Cheyette, who counsels historians to "consider feudalism a *technique*, rather than an *institution*, . . . a technique involving above all a relation of personal dependence and service normally sealed by the grant of a dependent tenure or some other form of material support, and confined to that group of professional warriors who in time become the nobility, the *miles* [sic], the *domini*—a technique used to achieve certain purposes in certain places at certain times." "Some Notations on Mr. Hollister's 'Irony,'" 12.

³³ Joseph R. Strayer, "Feudalism in Western Europe," in Rushton Coulborn, ed., *Feudalism in History* (Princeton, 1956), 16, reprinted in Cheyette, *Lordship and Community*, 13. A similar definition appears in a lecture presented by Strayer in 1963 and published four years later as "The Two Levels of Feudalism," in Robert S. Hoyt, ed., *Life and Thought in the Early Middle Ages* (Minneapolis, 1967), 52-53, reprinted in Joseph R. Strayer, *Medieval Statecraft and the Perspectives of History: Essays by Joseph R. Strayer*, ed. John F. Benton and Thomas N. Bisson (Princeton, 1971), 63-65. In this essay Strayer maintains that a broader definition, referring to economic and social conditions, "in fact defined nothing," and he asserts that "the narrow, military definition of feudalism" ("a way of raising an army of heavy-armed cavalymen by uniting the two institutions of vassalage and the fief"), while laudably precise, is "too limited" to be useful, since, if defined in this way, feudalism "would have little historical significance." In Hoyt, *Life and Thought*, 52-53 (in Strayer, *Medieval Statecraft*, 64-65). See also Strayer's comments in *Feudalism* (Princeton, 1965), 13-14. This point of view was again expressed, in modified form, in an essay Strayer published in 1968: "The Tokugawa Period and Japanese Feudalism," in John W. Hall and Marius B. Jansen, eds., *Studies in the Institutional History of Modern Japan* (Princeton, 1968), 3, reprinted in Strayer, *Medieval Statecraft*, 90. In this

decided that this definition was defective,³⁴ and in 1965 he advanced one that included a military as well as a political element. Then he presented as "the basic characteristics of feudalism in Western Europe . . . a fragmentation of political authority, public power in private hands, and a military system in which an essential part of the armed forces is secured through private contracts." Thus feudalism was seen not only as "a method of government" but also as "a way of securing the forces necessary to preserve that method of government." It seems clear, however, that Strayer still considered the jurisdictional element fundamental, for in concluding his discussion he wrote that "a drive for political power by the aristocracy led to the rise of feudalism."³⁵

Other approaches to the problem of defining feudalism have been taken. In 1953 Georges Duby stated a bit hesitantly that "what one refers to as feudalism" (*ce qu'on appelle la féodalité*) should be understood to have two aspects, the political—involving the dissolution of sovereignty—and the economic—the constitution of a coherent network of dependencies embracing all lands and through them their holders.³⁶ Thus he created a bridge

essay Strayer states that "in political terms, feudalism is marked by a fragmentation of political authority, private possession of public rights, and a ruling class composed (at least originally) of military leaders and their followers." Note that this definition, explicitly couched "in political terms," does not exclude the possibility of formulating other definitions phrased in different terms.

³⁴ This modification resulted from a reorientation of approach that occurred in 1962 and 1963, when Strayer established his concept of two levels of feudalism. In reviewing Marie Fauroux's *Recueil des actes des ducs de Normandie, 911-1066* (Caen, 1961), Strayer commented that "many scholars have failed to see that there were really two feudalisms—the feudalism of the armed retainer or knight, and the feudalism of the counts and other great lords who were practically independent rulers of their districts. The two feudalisms began at different times and under different circumstances, and it was a long time before they were fully meshed together." In *Speculum*, 37 (1962): 608. Although Strayer did not explicitly define feudalism, his discussion revealed that "Norman feudalism of the classic type" required the holding of "land in return for a definite quota of military service." "Knights and other vassals" were important not only "for military purposes" but also as "part of the governing group," whose aid and counsel the duke needed to rule effectively, and who possessed local administrative authority (pp. 608-09). It is hard to reconcile this analysis with a definition of feudalism that emphasizes the disintegration of central authority and the consequent distribution of political power among numerous members of a ruling group, and in 1963 Strayer acknowledged that in Normandy political fragmentation—an essential element of the political definition of feudalism he described in the same essay as the original and "best" definition—was tardy and incomplete. "Two Levels of Feudalism," in Hoyt, *Life and Thought*, 51-52, see also 63-65 (in Strayer, *Medieval Statecraft*, 63, see also 74-75). In addition see Strayer, *Feudalism*, 39. Even outside Normandy it was not until the eleventh century—and then not consistently and regularly—that the lower as well as the higher social and military orders distinguished by Strayer can be said to have exercised independent political power. With the inadequacy of the political definition of feudalism exposed, it must have become evident that some additional element or elements would have to be added to produce a satisfactory definition of the term.

³⁵ Strayer, *Feudalism*, 13, 74. Note, too, that in "The Tokugawa Period," published in 1968, Strayer still laid heavy emphasis on the political aspect of feudalism.

³⁶ Georges Duby, *La société aux XI^e et XII^e siècles dans la région mâconnaise* (Paris, 1953), 643, the corresponding page in the reprint (Paris, 1971) is 481. Duby's evasive approach to the word *féodalité* reappears in his book *Guerriers et paysans, VII^e-XII^e siècle: Premier essor de l'économie européenne* (Paris, 1973). Here he uses terms reminiscent of those he employed in 1953 as he refers to "ce que les historiens ont coutume d'appeler la féodalité" (p. 179). Calling it "un mouvement de très grande amplitude," he does not define it precisely and explicitly, although he says that it was characterized by "la décomposition de l'autorité

of sorts, reconciling the definitions of Strayer and Ganshof. Later, however, Duby turned from government and land to mentalities, and in 1958 he suggested that feudalism might best be considered

a psychological complex formed in the small world of warriors who little by little became nobles. A consciousness of the superiority of a status characterized by military specialization, one that presupposes respect for certain moral precepts, the practice of certain virtues; the associated idea that social relations are organized as a function of companionship in combat; notions of homage, of personal dependence, now in the foreground, replacing all previous forms of political association.³⁷

Definitions of feudalism abound, and student and scholar have available to them broad ones that lump together numerous facets of medieval society and narrow ones that center on carefully chosen aspects of that society—tenurial, political, military, and psychical. The possibilities for bewilderment and dispute are dizzying, particularly since a single author's interpretation of the term can undergo marked shifts.

Another difficulty posed by feudalism and its system is the fact that those employing the terms, in whatever sense they use them, are constantly found qualifying and limiting the extent to which they believe them applicable to any particular time and locality in medieval Europe. Marc Bloch writes,

In the area of Western civilization the map of feudalism reveals some large blank spaces—the Scandinavian peninsula, Frisia, Ireland. Perhaps it is more important still to note that feudal Europe was not all feudalized in the same degree or according to the same rhythm and, above all, that it was nowhere feudalized completely.

Nostalgically, and with regret only a confirmed Platonist could harbor, he concludes, "No doubt it is the fate of every system of human institutions never to be more than imperfectly realized."³⁸

While Robert S. Hoyt could write of the growth and development of feudalism and could state that by the mid-eleventh century "an essentially feudal society had emerged throughout western continental Europe," he felt obliged, first, to deny that there was a "'feudal system' common to all Europe," and second, to assert that "there were endless diversity and variety."³⁹ In the introduction to *Feudalism* Ganshof notes that he proposes

monarchique" and coincided with the development of a new sort of warfare and the establishment of a new conception of peace; he discusses "un système économique que l'on peut, en simplifiant, appeler féodal"; he concludes that "au plan de l'économie, la féodalité n'est pas seulement la hiérarchie des conditions sociales qu'entend représenter le schéma des trois ordres [elsewhere described as le clergé, les spécialistes de la guerre, et les travailleurs], c'est aussi—et d'abord sans doute—l'institution seigneuriale" (pp. 179, 184, 185, 187, 191). Thus, on the economic plane, Duby substitutes the development of the lordship for the coherent network of dependencies that he stressed in 1953.

³⁷ Georges Duby, "La Féodalité? Une mentalité médiévale," *Annales: Économies, Sociétés, Civilisations*, 13 (1958): 766. See also the comments of J. M. Wallace-Hadrill in a review of Bloch's *Feudal Society*, in *English Historical Review*, 78 (1963): 117.

³⁸ Bloch, *Société féodale*, 2: 248, 249 (*Feudal Society*, 445).

³⁹ Robert S. Hoyt, *Europe in the Middle Ages* (2d ed.; New York, 1966), 190–96, 185.

to study feudalism mainly as it existed in France, in the kingdom of Burgundy-Arles and in Germany, since in these countries its characteristics were essentially the same, and to concentrate on the regions lying between the Loire and the Rhine, which were the heart of the Carolingian state and the original home of feudalism. Further afield, in the south of France and in Germany beyond the Rhine, the institutions that grew up are often far from typical of feudalism as a whole.⁴⁰

In his foreword to the book, F. M. Stenton praises Ganshof's self-imposed limitations and suggests that they result from a realization "that social arrangements, arising from the instinctive search for a tolerable life, vary indefinitely with varieties of time and circumstance." While it is easy to agree with Stenton that students should be disabused of the idea that "an ideal type of social order" dominated Western Europe, it comes as something of a shock to find him readily accepting the doctrine that in the huge area on which Ganshof focuses a single "classical feudalism" was to be found.⁴¹ The expectation of infinite variety in social arrangements seemingly ends for Stenton at the Loire and the Rhine, a good safe distance from the Thames.

The variety of definitions of feudalism and the limitations imposed on their relevance are confusing. Equally disconcerting is the pervasive tendency on the part of those who use the word to personify, reify, and to coin two words, occasionally "bacterialize," and even "lunarize" the abstraction. How often does one read that feudalism, like a virus, spread from one area to another, or that, later on, it slowly waned. In a single study feudalism is assigned a dazzling array of roles. It is found giving birth, being extremely virile, having vitality, being strong, knowing a long tradition, being successfully transplanted, surviving, being replaced, teetering, being routed, declining and falling, and finally dead and in its grave. Another author sees it destroying the Frankish Empire and making a clean sweep of outmoded institutions. For another it makes onslaughts on the power of the kings of France and England; "les forces féodaux" end the confusion of spiritual and temporal authorities. Still another work reassuringly attributes a home to feudalism, which is said to have exercised, rather adventurously, "paralyzing action" over "many forms of royal activity," and, more decorously, to have been "introduced into England in its French form" by the duke of Normandy.⁴² In concluding *Seigneurie et féodalité* Boutruche in fact triumphantly proclaims it madness to consider feudalism an abstraction. "In

⁴⁰ Ganshof, *Feudalism*, xvii.

⁴¹ Stenton, foreword to *ibid.*, vii–viii.

⁴² See Bryce D. Lyon, *From Fief to Indenture: The Transition from Feudal to Non-Feudal Contract in Western Europe* (Cambridge, Mass., 1957), 272–73; Georges Duby, *Adolescence de la Chrétienté occidentale, 980–1140* (Geneva, 1967), 61, 83. The corresponding pages in the English edition—translated by Stuart Gilbert and entitled *The Making of the Christian West, 980–1140* (Geneva, 1967)—are 61, 83. See also Bloch, *Feudal Society*, 59, 142, 443, where the statements found in *Société féodale*, 1: 95, 221 and 2: 245, are sometimes given a rather free interpretation. Finally, see Ganshof, *Feudalism*, xvii, 54, 59, 61.

actuality, it is a person. . . . Feudalism is medieval. . . . It is the daughter of the West."⁴³

Another problem is the inclination to employ the idea of fully developed, classical, or perfectly formed feudalism as a standard by which to rank and measure areas or societies. Territories are regularly divided into categories: some highly or thoroughly feudalized; others never, gradually, or only partly feudalized.⁴⁴ Non-European countries are evaluated in this manner, and the standard has often been applied to Japanese modes of social and political organization.⁴⁵ Such assessments can also be made of institutions. The Church in Norman Italy, for instance, has been judged "never feudalized to the same extent as . . . the Church in Norman England."⁴⁶

These examples all involve inanimate phenomena, geographical or institutional, but it is also possible to attribute to an individual or a group the aim of achieving complete feudalization or of introducing an articulated feudal system and then judge the person or group a success or failure in achieving this hypothesized objective. The precise nature of the goal would naturally depend on how the historian making the attribution defined feudalism or feudal system, but such assessments immediately imply that the person or group in question consciously planned and then attempted to implement a system based primarily on the granting of fiefs but also involving the establishment of a graded hierarchy of status and command and the delegation of sovereign power. D. C. Douglas transposes feudalism from the realm of the abstract into a concretely human framework when he says that in England William the Conqueror "was concerned to establish a completed feudal organization by means of administrative acts" and when he indicates that the conquest of England enabled William to realize the "feudal organization in Normandy." Before 1066, Douglas says, the Normans were "as yet unorganized in any rigid feudal scheme," the feudal structure "had not yet been fully formed," "the structure of Norman society had [not] as yet been made to conform to an ordered feudal plan."⁴⁷ A

⁴³ "La féodalité est présentée parfois comme une abstraction. Folie! En vérité, c'est une personne. . . . La féodalité est médiévale. . . . Elle est fille de l'Occident." Boutruche, *Seigneurie et féodalité*, 297.

⁴⁴ Lyon, *From Fief to Indenture*, 23-24; Joseph R. Strayer, "The Development of Feudal Institutions," in Marshall Clagett, Gaines Post, and Robert Reynolds, eds., *Twelfth-Century Europe and the Foundations of Modern Society* (Madison, 1961), 79, reprinted in Strayer, *Medieval Statecraft*, 78-79.

⁴⁵ Bloch, *Société féodale*, 2: 250-52 (*Feudal Society*, 446-47); Strayer, "The Tokugawa Period." For hesitations expressed by Ganshof and by Bloch himself concerning the validity of this approach, see Ganshof, *Feudalism*, xv-xvi; Bloch, *Société féodale*, 2: 242 (*Feudal Society*, 441), and *Apologie*, 89 (*Historian's Craft*, 175-76).

⁴⁶ Douglas, *Norman Achievement*, 176.

⁴⁷ Douglas, *William the Conqueror*, 281, 98, 283, 96, 104; see also note 32 above. See the more convincing analysis presented by Strayer in his review of Fauroux's *Recueil des actes*, 609-10. Like Douglas, however, Strayer concludes that although "Norman feudalism of the classic type was not fully developed until the second half of the eleventh century . . . it was William the Conqueror, more than any other ruler, who gave it definitive form." Note the warnings given by Richardson and Sayles against assuming that William had any "grand designs or well devised plans." *Governance of Mediaeval England*, 71. For a clearly integrated account

similar transformation of abstract model into consciously held goal occurs as Christopher Brooke asserts that "only in the Norman and the crusading states, colonized in great measure from the homeland of French feudalism, did one find any attempt to live up to a conception of feudalism as coherent as that of northern France."⁴⁸

APPRAISING IN TERMS OF AN IDEAL STANDARD need not involve making value judgments, but such assessments are ordinarily expressed in value-loaded terms. To say that a person or a group is attempting to live up to or realize a standard certainly suggests virtuous dedication on the part of the people in question. To declare that a country which is not feudalized is lagging behind is to indicate that the area is in some sense backward. Even more evidently evaluative are such expressions as decayed, decadent, and bastard feudalism, all of them implying a society's failure or inability to maintain pure principles that were once upheld.⁴⁹ One is occasionally struck by a rather sentimental regret that the societies, individuals, and groups which might have been encouraged by high marks to persevere or shamed by low ones into exerting an additional push are unable to benefit from them. Even if formulated in value-free terms, analyses of societies on the basis of their conformity to or deviation from a norm offer little insight into the societies themselves, however much the process of comparison may stimulate and challenge the ingenuity of historians. To produce helpful insights, comparative history must involve the examination of the widest possible range of elements, not those idiosyncratically dubbed essential by the historians devising the standard to be applied.

Asserting that individual rulers actively and consciously aimed at establishing feudalism and judging them in terms of this aim is, at another level, equally misconceived and misleading. That William the Conqueror, the Normans, and the Crusaders wanted to establish control within the areas they conquered as effectively as circumstances permitted is, I think, unquestionable; that they used and molded the institutional forms and arrangements with which they were familiar and which were available to them is equally undeniable. To suggest, however, that they operated on the basis of a definite, preconceived scheme focused primarily on the fief, and to measure their accomplishments by such a standard, is to give a distorted, simplistic picture of their actions and policies, projecting into the minds of people who dealt creatively and flexibly with numerous options and who

of William's accomplishments—which only once mentions the adjective "feudal"—see D. C. Douglas, "William the Conqueror: Duke and King," in Dorothy Whitelock, D. C. Douglas, C. H. Lemmon, and Frank Barlow, *The Norman Conquest: Its Setting and Impact* (London, 1966), 45–76; see p. 65 for "feudal."

⁴⁸ Christopher Brooke, *Europe in the Central Middle Ages, 962–1154* (New York [1963]), 100.

⁴⁹ See the comments of K. B. McFarlane, "'Bastard Feudalism,'" *Bulletin of the Institute of Historical Research*, 20 (1943–45): 161–62.

manipulated a variety of institutional devices to achieve their purposes a degree of calculation, narrowness of vision, and rigidity that the surviving evidence does not suggest characterized them and in which even a contemporary management specialist might have difficulty believing.

What of the other virtues attributed to feudalism as a means of comprehending medieval social and political life? As far as pedagogy is concerned, students should certainly be spared an approach that inevitably gives an unwarranted impression of unity and systematization and unduly emphasizes, owing to the etymology of the word, the significance of the fief. Even if historians agreed to define feudalism as feudal society and included within its scope all facets of social and political development, the practical problem would remain. There are other, more basic, disadvantages. To advocate teaching what is acknowledged to be deceptive and what must later be untaught reflects an unsettling attitude of condescension toward younger students. Furthermore, not only does such a procedure waste the time of teacher and student, but its supporters apparently disregard the difficulty of, as a student of mine puts it, "'erasing' an erroneous concept or fact from the mind of a child who has been taught it, mistakenly or intentionally, at a lower school level." This student, Marie Heinbach, who teaches social studies in a New York junior high school, goes on to point out that "the difficulty becomes almost insurmountable when the amazing retentive powers of a young and impressionable child are considered. In addition, as the amount of time between the learning and unlearning of a concept increases, it becomes nearly impossible totally to correct the misconceptions that a student may have."⁵⁰ Experts who knowingly mislead their students appear to be unsure of their own ability to present a simplified account of the conclusions concerning medieval society that historians have now reached. Those of their students who do not progress beyond the introductory stage are denied the knowledge that most medieval historians study the actions and interrelationships of human beings rather than concentrating on the formulation and refinement of definitions of abstractions. Such students are never exposed to the problems of social and family structure and their corresponding etiquettes or to the problems of territorial loyalties and group attachments that historians are now examining. Presented with an abstract model and sternly cautioned against assuming its general relevance and applicability, only the staunchest will be motivated to pursue the individuals and groups lurking behind and beyond the ism.

For scholars the approach has equally little use. Applying an artificially fabricated standard in which certain components are divorced from the context in which they existed is essentially sterile. And those who investigate the workings of medieval society run the risk of having their vision narrowed, their perspective anachronistically skewed, and their receptivity

⁵⁰ This statement was made in an examination submitted on March 27, 1974.

to divergent data consequently blunted unless they firmly divorce themselves from the preconceptions and sets associated with the oversimplified models and abstractions with which they have been indoctrinated and which they themselves pass on to their students.

What of the indispensability of feudalism? Here a distinction must be made. While the creation of intuitive abstractions and simple Ideal Types can indeed be explained by invoking the infinite and confusing variety of human experience, it is quite another matter to suggest that the procedure is obligatory, necessary, or laudable. Alternative modes of classifying and describing exist and can be used. Again, attempting to justify the formulation and use of such models and abstractions by maintaining that scholarly and scientific terminology and common usage assume their existence is patently circular, avoiding as this argument does the obvious fact that scholarly terminology can be revised and common usage clarified. Far more appropriate to express regret and to apologize for measures attributable to the weaknesses and defects of human modes of expression and perception. Historians and social scientists can, like natural scientists, devise multifactor, heuristic models that encompass and account for the available evidence, are reformulated to include newly discovered data, and are not misleadingly labeled so as to suggest either system and conscious organization where none existed or the predominant importance of one element in a situation in which many elements are known to have been significant.⁵¹ Such multifactor models and descriptive, narrative accounts, which emphasize complexity and the unique, can convincingly be said to encourage fuller, less distorted, and hence more acceptable understanding of the past than any "one-sided accentuation of one or more points of view."⁵²

The contention that such general terms as "feudalism" are essential for intelligent discourse is also debatable, and those who advance this defense reveal their own discomfiture when they invoke other commonly used abstractions, such as "war" and "agriculture," to serve as buttressing middle elements. Intelligent discourse devoid of general abstract terms is, the argument runs, inconceivable. All abstractions—feudalism, war, agriculture—are similar in nature. Therefore the isms are indispensable if intelligent discourse is to occur. This chain of reasoning is, however, flawed in its second step, for there is an evident difference between, on the one hand, those collective descriptive abstractions arrived at by isolating common features of different phenomena similar enough to permit the use and assure the acceptance of single words to denote them, and, on the other

⁵¹ See, for the natural sciences, N. R. Hanson, *Observation and Explanation: A Guide to Philosophy of Science* (New York, 1971), 77–84; T. S. Kuhn, *The Structure of Scientific Revolutions*, vol. 2, no. 2 of the *International Encyclopedia of Unified Science* (2d ed.; Chicago, 1970), 100–02; George Gamow, *Thirty Years That Shook Physics: The Story of Quantum Theory* (Garden City, 1966), 155; and James D. Watson, *The Double Helix* (New York, 1969), 18, 38, 47, 49, 61, 83, 123.

⁵² Max Weber, *The Methodology of the Social Sciences* (Glencoe, 1949), 90, quoted and discussed in Lane, *Introduction to Structuralism*, 25.

hand, those abstract analytic constructs formulated and defined as a shorthand means of designating the characteristics that the observers consider essential to various time periods, modes of organization, movements, and doctrines. To a degree to which the first type is not, the second sort of general term is inevitably and often intentionally affected by the theories and assumptions of the formulators and users. Disagreements over the exact meaning of "war" or "agriculture" do occur, but they can ordinarily be resolved by introducing greater precision and clarity into the definitions of the terms, whose core signification is not generally contested. In distinction, infinite disagreement about the meanings of the isms is possible and perhaps inevitable, since the terms were not devised to designate the basic elements of fundamentally similar classes of phenomena but rather to refer to selected elements of complex phenomena, the choice of which inevitably involves the idiosyncratic value judgments of the terms' inventors and employers. Thus, however easy it is to say what the words "fief," "capital," and "merchant" mean, it is another thing entirely to seek consensus on the definitions of "feudalism," "capitalism," and "mercantilism," precisely because of the subjective nature of the definitions of these words. To raise the level of discourse and make it truly intelligent, there should be general agreement to consider the isms no more than the artificialities they are.

DIRECT EXPRESSIONS OF DISCONTENT with the term "feudalism" have increased in number and strength over the past two decades. From time to time there has seemed reason to hope that, with a resounding whoop, historians would join together, following the example of the National Assembly, to annihilate the feudal regime and, with the good members of the Legion of Honor, agree "to combat . . . any enterprise tending to reestablish it."⁵³ At least partly responsible for the mounting volume of protest is the reorientation of perspective that took place in 1953 with the publication of two remarkable books, one French and one English, both dealing with the political and social life of Western Europe in the tenth through the twelfth centuries, both concerned with individuals rather than abstractions, and both avoiding the medieval isms.

Of these books the purest—in that it does not, as far as I can tell, contain the word "feudalism"—is Richard W. Southern's study, *The Making of the Middle Ages*. In a section devoted to "The Bonds of Society" Southern presents an illuminating introduction to the political life of the eleventh and twelfth centuries by concentrating on a single, "unusually instructive" example of "what happened where the control exercised by the past was least effective, and where the disturbing elements of trade, large

⁵³ Bloch, *Société féodale*, 1: 2-3 (*Feudal Society*, xvii); see also Boutruche, *Seigneurie et féodalité*, 20-21.

towns and active commercial oligarchies were not conspicuous." Discussing the emergence of the county of Anjou, Southern uses such abstract terms as "the disintegration of authority" and "the shaping of a new political order." He writes, generally, of "an age of serious, expansive wars waged by well-organized and strongly fortified territorial lords." The term "feudal" is sometimes used in a general sense, in contexts in which it clearly implies more than connection or involvement with the fief. When the term is given this broader meaning, however, it seems to be so used out of force of habit rather than from any conscious conviction that it is the most appropriate and meaningful word to be found. "The art of feudal government" and "the early feudal age," neither phrase explicitly defined by Southern, are reminiscent of Bloch's *La Société féodale*, a book Southern recommends, and they strike a jarring note of vagueness and imprecision in a discussion otherwise notable for its concreteness. On the few other occasions when Southern employs the term "feudal" in this general way, alternative expressions that he devises to describe the phenomena in question are strikingly more informative. "Knightly" is one of these alternative terms, and, on a more extended scale, "the straightforward feudal-contract view of society" is far less subtle and suggestive than his evocative description of an "imagination . . . circumscribed by the ties of lordship and vassalage, by the recollection of fiefs and honours and well-known shrines, by the sacred bond of comradeship."⁵⁴

Only a small portion of Southern's book is devoted to social and political ties and the exercise of governmental power, but Georges Duby, in his study of the Mâconnais in the eleventh and twelfth centuries, dedicates an entire volume to these subjects. Hence it is all the more noteworthy that in his index, as in Southern's, there is no reference to *féodalité*, although the index does list the indisputably acceptable terms *feudataire*, *fidèle*, *fidélité*, and *fief*, which are derived from and accurately reflect the terminology and usage of the eleventh and twelfth centuries.⁵⁵ Duby's avoidance of the term *féodalité* is consistent with his avowed purpose in writing his book. In his preface he announces that he is studying a small province in order to approach human beings directly, without isolating them from their milieu.⁵⁶ This he does, describing first the state of society in the Mâconnais at the end of the tenth century, then the period of independent castellanies from

⁵⁴ Richard W. Southern, *The Making of the Middle Ages* (New Haven, 1953), 90–91, 80–81, 87, 86, 262, 55, 241. When Southern mentions "the straightforward feudal-contract view of society," he associates the term "feudatory" with the "holding [of] land in return for military service" (p. 55); see also p. 56 for a reference to "the formula of feudal government" and p. 242 for "feudal custom" and "feudal etiquette"; for "knightly ideal" see p. 241; see also pp. 55, 243.

⁵⁵ Duby, *Société aux XI^e et XII^e siècles*, 666 (reprint, 501).

⁵⁶ "J'ai volontairement conduit mes recherches dans le cadre étroit d'une petite province. La méthode des monographies régionales permet en effet d'approcher directement les hommes sans les isoler de leur milieu." *Ibid.*, ix (reprint, 7). In his conclusion, Duby again describes his approach: "Pour approcher de plus près les hommes, nous avons concentré notre attention sur une toute petite région" (p. 644 [reprint, 482]).

980 to 1160, and finally the movement between 1160 and 1240 from castellany to principality. His conclusions are significant, first because of the wealth of data on which they are founded but even more because the Mâconnais lies within—if at the southern extreme of—the area between the Loire and the Rhine where countless scholars have seen “classical feudalism” emerging, and also because its history does not exemplify the characteristics associated with this development.

Stressing the survival of comital power and superiority until the end of the tenth century, Duby shows that among the higher ranks of society the ties of fidelity linking those agreeing to some sort of mutual support were vague and imprecise, like family ties, and can best be described as confirming a relationship of *amicitia*. As the count's power declined and as that of the castellans increased, bonds of dependence among the higher classes became more important, and grants of land were used to solidify the ties until by 1075 land outweighed loyalty as their determinant. Obligations were still indefinite, however, and military service was not a significant component. Between men of unequal status, dependent relationships were closer, but the strength and meaning of these ties were limited by the small value of the fiefs that lords gave their followers, who generally possessed large allodial holdings, and by the multiplicity of the ties. According to Duby, “feudal institutions”—by which he apparently means not only fiefs but also homage and vassalage—had only superficial importance. They constituted a sort of superstructure that formalized without affecting pre-existing relationships.

Feudal institutions were adapted to the previous structure of the higher class without significantly modifying it. Between great lords or knights, homage is a simple guarantee, an agreement not to harm; between a small noble and a powerful one, it is a true dedication, an agreement to serve. Vassalage and the fief, customary practices born in private usage, organized the relations that unequal division of wealth and power had already determined; they created no additional ones. In eleventh-century Mâconnais, there was no pyramid of vassals, there was no feudal system.⁵⁷

Duby concludes that for the higher classes “feudalism was a step toward anarchy,” but by this he evidently means not that any ill-conceived and abortive attempt had been made to create harmony by introducing homage, vassalage, and the fief, but that the links ordered by these institutions were not strong or meaningful enough to serve as effective restraints. These were instead provided by the teachings and intervention of the Church, by family bonds, and by a variety of oaths. Thus, “although violent and disturbed, the world of lords was not anarchic.”⁵⁸ In this period the nobility exercised

⁵⁷ *Ibid.*, 94–116, 140–41, 172, 177–85, 194–95, see also 185–93, 291 (reprint, 93–108, 124–25, 149, 153–58, 164–65, see also 158–64, 235–36).

⁵⁸ *Ibid.*, 204 (reprint, 170). For a full discussion of these restraints see pp. 196–204 (reprint, 165–70).

for their own benefit governmental powers over the lower classes, but their actual control over land did not increase.⁵⁹

In the late twelfth and early thirteenth century the economy of the Mâconnais was transformed, and the king of France, long absent from the area, reappeared there. Economic pressures and royal policy produced a proliferation of ties of dependence and a marked decrease in allodial holdings; concomitantly, services may have become more definite and heavier. As far as justice was concerned, "the peace of the prince replaced the peace of God," and judicial procedures developed in the eleventh century were regularized and made more effective.⁶⁰

Duby occasionally uses the word *féodalité*, but the term has no central significance in his book, thanks to his determination to focus on individuals and their actions. In his general conclusion he relates his findings to his own definition of feudalism, which, as has been seen, involves the disintegration of central authority and the development of an inclusive web of dependencies. In the Mâconnais, he reminds his readers, these two characteristics appeared successively rather than simultaneously, since in the eleventh and twelfth centuries, when most lands were freely held, jurisdictional powers were in the hands of private lords, and in the thirteenth century, when most lands were involved with dependent relationships, sovereignty reappeared in the persons of kings and princes.⁶¹ Duby refuses to comment on the districts outside the Mâconnais, and he calls for additional local studies. Nonetheless he notes that "the society of the Mâconnais did not evolve in isolation." Pointing out that the Mâconnais was "a province of feudalism with marked individual characteristics,"⁶² he implicitly suggests that other areas lying within the fabled heartland of feudalism were equally distinctive.

Duby does not openly attack the use of the concept of feudalism, nor does he denounce the idea that institutions in the Loire-Rhine region were similar enough to be described as a single phenomenon. Still, his conclusions demonstrate the futility of generalizations that are not based on the study of successive generations of human beings inhabiting a restricted area. They also suggest the inappropriateness of descriptive terms that fail to convey a sense of the variety of experience and development to be found throughout Western Europe between the tenth and the late twelfth centuries. When I once asked Monsieur Duby what difference there was between his book on the Mâconnais and Ganshof's study of feudalism, he

⁵⁹ *Ibid.*, 329–30 (reprint, 261–62). For the close connection Duby now posits between the development of the ideology of the peace of God and "les premiers phases de la féodalisation," see note 36 above and Duby, *Guerriers et paysans*, 185.

⁶⁰ Duby, *Société aux XI^e et XII^e siècles*, 473–569, 571 (reprint, 361–427, 429).

⁶¹ Using Bloch's periodization, Duby concludes that only in this sense could there be said to have been "two feudal ages." The second age—a time of fiefs, censives, and feudal principalities—contrasted with the earlier age of independent castellanies, and Duby believes that it began no earlier than 1160 and that it ended in 1240. *Ibid.*, 642–43 (reprint, 481–82).

⁶² *Ibid.*, 644 (reprint, 482).

replied with a modest shrug of the shoulders, "Toute la différence du monde, Madame." His own book is a testimony to his conviction that understanding the workings of medieval society necessarily involves exploring the intricate complexities of life rather than elaborating definitions and formulas designed to minimize, simplify, and, in the last analysis, obscure these complexities.⁶³

SOUTHERN AND DUBY had their predecessors—historians who probed beyond or disregarded the construct feudalism and who concentrated on analyzing and describing the many different ties and modes of dependency binding human beings to one another. Unquestionably, the work of Duby and Southern has acted as an additional, powerful stimulus, prompting more scholars to study the actual functioning of society in different areas. In general, however, and certainly in works directed at a popular rather than a scholarly audience, the situation remains much the same as it has been, and there is virtually universal resistance and opposition to abandoning the term "feudalism" and to confining the word "feudal" to its narrow sense—"relating to fiefs." The reservations regarding the use of the generalized constructs implicit in the books of Southern and Duby have not yet had the widespread effect that might have been hoped.

Exceptions do, of course, exist. In the books he has published since 1953 Southern has consistently employed his brilliant descriptive techniques and has assiduously avoided the term "feudalism."⁶⁴ R. H. C. Davis is now following a similar path, having apparently undergone something of a conversion. In the history of medieval Europe that he wrote in 1957 the word "feudalism" occasionally appears. England after William's conquest is called "the best and simplest example of a feudal monarchy." The index refers readers wishing to learn about "fully-developed feudalism" to pages Davis evidently considers relevant to this subject. How refreshing, then, to turn to an article

⁶³ See, however, his more restrained comments in "La Féodalité?" 765–66. Duby recommended Ganshof's study of feudalism as a guide and reference work but suggested that the very clarity, simplicity, and Cartesian rigor which are among its chief virtues may give the reader a false impression of order and regularity.

⁶⁴ See his book *Western Society and the Church in the Middle Ages* (Harmondsworth, 1970). Under the circumstances it is not difficult to forgive him for translating the word *homo*, which literally means no more than "man," as "vassal" in his edition of the *Vita Anselmi: The Life of St Anselm, Archbishop of Canterbury*, by Eadmer (Edinburgh, 1962), 111. Southern recently told me that he thinks "deplorable" not only the term "feudalism" but also the words "humanism" and "scholasticism." He said that he had never knowingly used the word "feudalism" to refer to actual conditions in the Middle Ages. He offered, however, a tentative and qualified defense of the word in *Medieval Humanism and Other Studies* (Oxford, 1970), 29. Southern's work suggests that he thinks the words "humanism" and "scholasticism" may have some practical value, however defective he may judge them on a theoretical plane. Medieval humanism is the central subject of his collected essays, and in a lecture, "The Origins of Universities in the Middle Ages," given at Philadelphia on April 8, 1974, Southern emphasized the importance of "scholasticism" and "scholastic" thought, calling the universities "the power house of scholasticism." In his conclusion, however, he warned that "European scholastic development" should be envisioned not as a single whole but as marked by diversity and variety.

on the Norman Conquest written ten years later and to find there a convincing analysis of William's accomplishments that contains no reference to the ism or its associated forms.⁶⁵

Southern and Davis are unfortunately in a minority. Far more numerous are the scholars who, while attacking the concept feudalism, still use the term and even encourage its propagation by suggesting new and better definitions. The contradictions in the work of Richardson and Sayles have already been discussed. Fully as puzzling is the case of Duby himself. Having implicitly questioned the aptness of the term in his study of the Mâconnais, he proceeded in 1958 not only to employ it but also, as has been seen, to advance an alternative definition, unusual and idiosyncratic, which he appears subsequently to have rejected. In a still later work, directed at a less scholarly audience, Duby employs the term *féodal* which, while undefined, clearly refers to something more general than the fief. It is found modifying such nouns as *éparpillement*, *forces*, *cours*, *princes*, and *seigneur*; a section of the book is entitled "Les féodaux," and the construct feudalism is several times personified.⁶⁶ A popular work published in 1973 shows that Duby's dedication to and reliance on the term have, in recent years, simply increased. He repeatedly refers to *féodalité* and uses the adjective *féodal* in a vague, indefinite way, and he goes so far as to designate the period from the mid-eleventh to the late twelfth century "les temps féodaux."⁶⁷ Saying that feudalism was characterized by the disintegration of monarchical authority and associating it with the institutions of the *seigneurie*, Duby presents feudalism as being implanted and established; he refers to feudalization, a feudal epoch, feudal society, feudal Europe, feudal peace, feudal structures, and a feudal economy and economic system.⁶⁸

Striking inconsistencies appear in Christopher Brooke's five-page discus-

⁶⁵ R. H. C. Davis, *A History of Medieval Europe from Constantine to Saint Louis* (London, 1957), s.v. "feudalism" in the index, and see also p. 127, where he enclosed the term "feudalism" in quotation marks; and see pp. 295, 414. Davis, "The Norman Conquest," *History*, 51 (1966): 279-86, reprinted in C. W. Hollister, ed., *The Impact of the Norman Conquest* (New York, 1969), 123-33.

⁶⁶ Duby, *Adolescence de la Chrétienté occidentale*, 60-61, 84. The translation of this book exaggerates these tendencies: see the corresponding pages, *Making of the Christian West*, 61-62, 83-84, and note that the section Duby entitled "Les féodaux" is called "Feudalism" in the translation. In *L'économie rurale et la vie des campagnes dans l'Occident médiéval* (Paris, 1962), Duby may refer to "la seigneurie des temps féodaux" (p. 379), but he generally avoids the term, and *féodalité* is not listed in the index. Note, however, that in the translation of the book published in 1968, "temps féodaux" becomes "the feudal period," and "rente seigneuriale" is transformed into "feudal rent." *Rural Economy and Country Life in the Medieval West*, tr. Cynthia Postan (London, 1968), 171, 232-59.

⁶⁷ Duby, *Guerriers et paysans*, 179-204, and see note 36 above.

⁶⁸ See also *ibid.*, 194 ("l'implantation de la féodalité"), 262 ("l'établissement de la féodalité"), 184 ("l'établissement des structures féodales"), 185 ("la féodalisation"), 278 ("l'époque féodale"), 192 ("la société féodale"), 201 ("l'Europe féodale"), 300 ("la paix féodale"), 184 ("les structures féodales"), 189 ("l'économie féodale"), 187 ("un système économique que l'on peut, en simplifiant, appeler féodal"). It is heartening to note that the review of Duby's *Guerriers et paysans* in the *Times Literary Supplement* does not contain the words "feudal" or "feudalism." Aug. 17, 1973, pp. 941-42.

sion of barons and knights in a book he published in 1963. Having begun by declaring that "few historical labels are more ambiguous than 'feudal'" and by proclaiming that he would therefore "use it as little as possible," having then warned that "it is doubtful whether [strict feudalism] ever existed outside the imaginations of historians," he proceeds, without defining the term "feudal," to use it, imprecisely and ambiguously, in writing of "the feudal bond," "feudal conceptions," "the feudal contract," "the feudal oath," and "feudal and quasi-feudal institutions." He also refers to "highly developed" feudalism, "classical feudalism," "French feudalism," and "strict feudalism." Finally he both reifies feudalism and uses the phrase "coherent feudalism" to designate a consciously formulated and adopted set of goals and principles.⁶⁹

THE HESITANCIES, CONTRADICTIONS, AND INCONSISTENCIES that have been reviewed—and that are wholly typical of statements found in the books on medieval society published in the past twenty years—clearly demonstrate how necessary it is to reassess the value of the words "feudal" and "feudalism." It must be admitted that there is little possibility of ridding the historical vocabulary of them, adopted as they have been by the scholarly community in general and by the economists in particular. The terms exist. They have been and probably will be used for many years. As words students know if they know nothing else about the Middle Ages, they cannot be avoided. But confrontation need not mean capitulation, for it is perfectly possible to instruct students at all levels to use "feudal" only with specific reference to fiefs and to teach them what feudalism is, always has been, and always will be—a construct devised in the seventeenth century and then and subsequently used by lawyers, scholars, teachers, and polemicists to refer to phenomena, generally associated more or less closely with the Middle Ages, but always and inevitably phenomena selected by the person employing the term and reflecting that particular viewer's biases, values, and orientations. Illustrations of the many meanings attached to "feudal" and "feudalism" can be given, and students with a flair for historiography can be encouraged to explore the eccentricities of usage associated with the terms.

Other students will be directed to the study of medieval society and politics, and they and their instructors will be faced with the necessity and challenge of finding an adequate means of describing the elements historians have investigated and should explore and the positive conclusions that have been reached.⁷⁰ Throughout, the terminology and word usage of those who

⁶⁹ Brooke, *Europe in the Central Middle Ages*, 95–96, 99–100. Brooke writes that "in its origin feudalism provided for the recruitment of vitally needed cavalry troops" (p. 100). See also pp. 1076–77 above. Note, too, that having just questioned the validity of the idea of "strict feudalism," Brooke scrupulously encloses "feudal" in quotation marks when he refers to "'feudal' means" of raising troops (p. 100).

⁷⁰ See Joseph R. Strayer, "The Future of Medieval History," *Medievalia et Humanistica*, n.s. 2 (1971): 182. Only since the appearance in 1968 of Fredric Cheyette's invaluable collec-

lived in the Middle Ages must be emphasized, and attention must be paid to the shifting meanings of key words, as well as to the gulf between actual practice and the formal, stylized records that have survived. Some elements will be pointed to as constants of general importance: the slowness and difficulty of communication, the general insecurity, the sluggish rate of technological change, and the reverence for tradition. The varying effects and significance of terrain, warfare, and violence must be emphasized. Stress must also be given to the resultant regional and diachronic variations in forms of government, modes of military organization, social and family structure, social mobility, the relationship between social class and function, styles of agricultural exploitation and commercial activity, and urban growth. Attention must be called to the different social and political relationships in which human beings were involved, to the ceremonies through which these relationships were fixed and manifested, and to the varying sorts of ties that superficially similar ceremonies could be used to create: bonds of obligation, fidelity, and support between sovereigns and their subjects, created and confirmed by oaths, pledges, and services; ties of loyalty, solidarity, and mutual assistance among people of similar and different social classes, formalized in communes, confraternities, guilds, leagues, and alliances, constituted through mutual undertakings that were sometimes left vague and sometimes clearly defined, solidified through privileges granted to and demanded by these groups; religious ties binding members of local congregations, regional churches, and similar faiths; ties of dependence forged between individuals or inherited from the past, sometimes involving friendship, sometimes service, sometimes protection, reinforced by gestures and oaths, resulting in benefits—material, monetary, territorial, social—to one or both parties; family bonds, revealed and consolidated in testamentary provisions, marriages, special festivities, and feuds and vendettas. The written and unwritten rules governing these ties and relationships must be considered, as must the ways in which and the different degrees to which these principles were systematized and enforced.

But to be properly understood, these elements must be observed as they developed, interacted, and changed, and thus the importance of presenting searching and detailed descriptions of areas characterized by different forms of governmental and social structure and organization and by different modes of development. Regions where strong monarchies developed and survived must be given as extensive consideration as areas where they disappeared, so that any given region—the Empire, England, Italy, Normandy, the Ile-de-France, the Mâconnais—will be considered neither abnormal nor typical but will be viewed as an instance of the varying ways human beings responded to similar and dissimilar circumstances, whose

tion of translated essays, *Lordship and Community*, has it been practically possible to direct beginning students to the recent literature in which this perspective on medieval society is reflected.

impact was conditioned by the total pasts of the people they affected. Those who are introduced to the study of medieval social and political life in this way will be far less likely than those presented with definitions and monistically oriented models to be misled about the conditions of existence in the Middle Ages. They will find it difficult to contrive and parrot simplistic and inaccurate generalizations about medieval Europe, and they may be challenged to inquire into subjects and areas as yet uninvestigated and to seek solutions to problems as yet unanswered.

The unhappiness of historians with the terms "feudal" and "feudalism" is, thus, understandable. Far less comprehensible is their willingness to tolerate for so long a situation often deplored. Countless different, and sometimes contradictory, definitions of the terms exist, and any and all of these definitions are hedged around with qualifications. Using the terms seems to lead almost inevitably to treating the *ism* or its system as a sentient, autonomous agent, to assuming that medieval people—or at least the most perspicacious of them—knew what feudalism was and struggled to achieve it, and to evaluating and ranking societies, areas, and institutions in terms of their approximation to or deviation from an oversimplified Ideal Type.

Despite the examples set by Southern and Duby some twenty years ago and followed in the interim by some scholars, historians have been generally loath to restrict the term "feudal" and discard the term "feudalism," particularly in dealing with general rather than specialized audiences. Feudalism's reign has continued virtually unchallenged, with ambivalence characterizing the attitudes of most historians toward the subject. The situation, however, can and should change. The arguments advanced to defend using the terms as they have been used in the past are weak, based as they are on vaguely articulated assumptions concerning the concept's utility as a verbal and intellectual tool, as a teaching device, or as a mode of evaluation—none of which is convincingly established. Similarly unsatisfactory are justifications founded on hypothesized requirements: the historian's need, as scientist, for abstractions like feudalism; the basic demands of discourse; or necessities created by the fundamental and seemingly insurmountable limitations of the human mind. Preferable alternative perspectives and terms exist, and there seems no reason to delay channeling all available energies to the study of human beings who lived in the past, thus putting an end to the elaboration of arid definitions and the construction of simplistic models. The tyrant feudalism must be declared once and for all deposed and its influence over students of the Middle Ages finally ended. Perhaps in its downfall it will carry with it those other obdurate *isms*—manorial, scholastic, and human—that have dominated for far too long the investigation of medieval life and thought.

Napoleon's Prefects

EDWARD A. WHITCOMB

IN ALL THE GREAT BODY of Napoleonic literature there exists not a single scholarly study of Napoleon's prefects. The creation of the prefectural corps was unquestionably one of Napoleon's greatest administrative achievements and certainly one of the most lasting. Napoleon's prefects governed France for fifteen years; they liquidated the Revolution; they established the basis of local administration in modern France; and they administered the largest and most successful European empire since Charlemagne. The texts of French, European, and administrative history devote a few lines or paragraphs to the prefects; the more specific studies of the period may contain a page or two on them. Yet, if one looks to the bibliography or the footnotes for a satisfactory reference on the prefects one is quickly disappointed. The accounts by J. Regnier and Jean Savant are often cited, but neither is scholarly, well researched, accurate, or documented.¹ A number of specific studies exist, biographies of a few of the prefects or of the administration in certain areas. These are useful in themselves but dangerous as a basis for generalization, the biographies because they concentrate on a few totally unrepresentative prefects, the regional studies because they are concentrated on the border areas of France, which are least representative of France as a whole. Nor are there sufficient local studies to permit generalization.²

The texts suggest a number of generalizations about the prefects—they were supposedly young; from a variety of backgrounds, especially the Revolutionary assemblies; from a place other than the department they administered; increasingly of noble birth; and were good administrators. These

This study was made possible by a grant from St. Francis Xavier University, Antigonish, Nova Scotia, Canada. I should also like to thank Professor John Bosher for his valuable comments and advice. Part of this article was read at the 1974 conference of the Society for French Historical Studies, Johns Hopkins University.

¹ J. Regnier, *Les Préfets du Consulat et de l'Empire* (Paris, 1907); and J. Savant, *Les Préfets de Napoléon* (Paris, 1958).

² Sixty years ago Aulard concluded his short study of Napoleon's administration with an appeal for more regional studies. In 1965 Jacques Godechot concluded his introduction to one such study with a similar plea: "We hope that this book will be the first in a long series of similar studies." See A. Aulard, *Études et leçons sur la Révolution française, septième série* (Paris, 1913); and J. F. Soulet, *Les Premiers Préfets des Hautes-Pyrénées 1800-1814* (Paris, 1965), preface by Jacques Godechot.

popular generalizations are partly true, partly false, have never been proven, and tell us little anyway. It would be valuable to know exactly who these prefects were: their social, political, functional, and geographical backgrounds; whether they were young or old; whether French or foreign; what happened to them after their employment as prefects; and how all these factors evolved during the Napoleonic period. But most valuable of all would be a clear and authoritative answer to the significant and complicated question of the quality of the prefectural corps as an administration. The general opinion is that the prefects provided excellent administration but that the quality of the corps deteriorated, especially after 1808 or 1810. This raises immediately the question of how quality is evaluated, a matter that Napoleonic historians have hardly considered, in spite of their repeated generalizations about the subject. This study will suggest a number of criteria of administrative quality and will argue that by these criteria the Napoleonic prefectural corps gradually and consistently improved. Such a conclusion is diametrically contrary to the prevailing opinion of the vast majority of historians.

Sixty years ago Aulard, the great historian of the French Revolution, said that Napoleon recruited his prefects from the moderates of the Revolution. He went on to identify thirteen conventionnels, fifteen constituants (three of whom were also conventionnels), eight members of the Legislative Assembly, and six from the Directory (one of whom was also in the Legislative Assembly). Yet, strange to say, Aulard's arithmetic seems to be faulty, for his evidence shows that only 41 of the first 97 prefects were Revolutionary politicians. This means that a majority (58 per cent) were not Revolutionary politicians, which is a flat contradiction of his statement. Later on Aulard suggests that there was no change in recruitment under the Empire, leaving the impression that a majority of the entire corps consisted of moderates from the Revolution.³ It is a significant commentary on Napoleonic historiography that these statements have gone unchallenged for half a century and have been repeated by the most notable historians of the period. Thus the best biography of Napoleon states that "they were nearly all moderate men of the Revolution."⁴ While Aulard exaggerated the conclusion but not the evidence, some historians have vastly increased the number of Revolutionaries. Aulard found only 41 politicians, but J. Siwek-Pouydesseau claims there were 71 former politicians among the first appointees.⁵ There is only one way this figure could be obtained, and that is by counting some prefects two or three times because they sat in more than one assembly.

Even if the evidence presented by Aulard and others were accurate,

³ Aulard, *Études*, 128–30.

⁴ F. M. H. Markham, *Napoleon* (New York, 1963), 81.

⁵ J. Siwek-Pouydesseau, *Le Corps Préfectoral sous la troisième et quatrième Républiques* (Paris, 1969), 12. Similarly, Godechot says that there were 76 Revolutionaries among the first appointees, which would be almost 80 per cent. Jacques Godechot, *Les Institutions de la France sous la Révolution et l'Empire* (2d ed.; Paris, 1968), 387.

which it is not, it would be irrelevant, since it only applies to the first 97 prefects and not to the entire Napoleonic corps of 281; the first prefects were the most unstable, one-third of them leaving the corps within the first three years. These Revolutionary prefects have taken on a special significance since Aulard's time, partly because some historians have come to regard them as the best administrators in the corps, partly because of the exaggeration in their numbers.⁶

Clearly, a significant number of prefects had been Revolutionary politicians, and they have attracted a considerable amount of attention from historians. Unfortunately, a discussion of them involves some problems of definition. A careful reading of the job applications throughout the period reveals that around 1794 the applicants were ardent supporters of the Revolution. By 1799 the same people had become moderate supporters. By 1810 they claimed to have accepted the ideas of the Revolution but never its excesses; and by 1814 they had become counterrevolutionaries. Between Robespierre and the Comte d'Artois there were a million shades of difference, and the identification of Revolutionaries can become purely subjective. And if the definition of a Revolutionary has its problems, then the identification of moderates is altogether impossible. It is best to avoid the word moderate, and define a Revolutionary politician as anyone who belonged to a national assembly between 1789 and 1799.

Eighty-three of Napoleon's prefects (30 per cent) had belonged to a Revolutionary assembly.⁷ They included 16 members of the National Assembly, 13 from the Legislative Assembly, and 19 members of the Council of Five Hundred or of Ancients. Twenty-four sat in the Convention, most of these in one or more other assemblies. In addition, five members of the National Assembly and six of the Legislative Assembly also sat in the assemblies of the Directory. If one ends the Revolution at Thermidor, as many historians do, then only 64 (22 per cent) of the prefects were Revolutionaries. We shall assume, however, that the Revolution lasted until 1799 and thus exclude only the Napoleonic assemblies from the count. Forty of these 83 politicians entered the corps in 1800, while most of the remaining ones were appointed in the early years. Naturally their numbers gradually

⁶ See, for example, the works of such historians as George Lefebvre, Geoffroy Bruun, and Alfred Cobban.

⁷ My evidence is based on a study of about 90–95 per cent of Napoleon's prefects and includes their place and date of birth; their social, political, and functional background; their administrative career; and their future. The remaining five per cent were the least important and served for only short periods. The evidence comes from the personnel dossiers of the prefects, series F1b1 150–178 (232 cartons) in the Archives Nationales (Paris), plus the following biographical dictionaries: *Biographie Nationale de Belgique* (Brussels, 1866); *Biographie Universelle ancienne et moderne* (2d ed.; Paris, 1843); *Dictionnaire Historique et Biographique de la Révolution et de l'Empire* (Paris, 1899); *Dictionnaire des Parlementaires français de 1789 à 1889* (Paris, 1889–90); *Galerie administrative ou biographie des préfets depuis l'organisation des préfectures jusqu'à ce jour* (Aurillac, 1839); and G. Six, *Dictionnaire biographique des généraux et amiraux français de la Révolution et de l'Empire* (Paris, 1934). Information readily available in biographical dictionaries will not be documented in the text.

TABLE 1. POLITICAL BACKGROUND: NUMBER AND PROPORTION OF REVOLUTIONARY POLITICIANS^a

<i>Years</i>	<i>Number</i>	<i>Proportion</i>
1800	40	40%
1801	46	47%
1802	50	46%
1804	47	44%
1805	47	43%
1806	45	42%
1807	42	38%
1808	41	37%
1809	37	33%
1810	35	30%
1811	28	22%
1812	28	22%
1813	29	23%
1814	24	19%

^a The percentages in all the following tables must be explained. The size of the prefectural corps and the number of departments were not necessarily the same, because a department was unoccupied at some times and, at other times, a new prefect would be appointed before his predecessor retired. The tables, unless otherwise indicated, are based on the number of prefects or the number about whom information is available.

decreased owing to the effects of death, retirement, promotion, transfer, or dismissal. Their proportion decreased even faster because of the expansion of the corps from 97 to 131. Table 1 gives their absolute number and proportion as of January 1 of every year, or April 1800 when the first appointments were made. Although the Revolutionary politicians constituted a significant but diminishing part of the prefectural corps, they were never in a majority.

When discussing the political origins of the prefects, writers often identify emigrés or royalists as being at the opposite end of the political spectrum from the Revolutionary politicians. Alfred Cobban tells us that Napoleon appointed more and more "secret royalists," and other historians mention royalists and emigrés in the corps.⁸ Some of these can readily be identified. Joseph Arbaud-Jouques, the son of a *président à mortier* of the parlement of Aix, emigrated and served in Condé's army. Alexandre de Larochevoucauld and Frédéric LaTour du Pin were among the most famous of the emigrés. Benôit Capelle hastily surrendered Geneva to the Allies in 1813, organized resistance to Napoleon during the One Hundred Days, and served in the First and Second Restorations. Vincent Viennot de Vaublanc was retained by the First Restoration, defended the Bourbons in 1815, and accompanied Louis XVIII to Ghent in 1815.

It would be most useful to identify the total number, proportion, and changing strength of the emigrés and royalists. Unfortunately this is impossible. Donald Greer has pointed out that the emigrés were people of

⁸ Alfred Cobban, *A History of France 1799-1871* (2d ed.; Harmondsworth, 1965), 2: 26.

all classes and occupations, mostly from the border areas of France, who were forced to leave for a variety of reasons.⁹ Some of them were active counter-revolutionaries, but these political émigrés cannot be separated from the rest, and a few of them had been Revolutionaries. Some of the political émigrés did not remain loyal to the king: Jean Grégoire du Colombier was a *fédéralist*, an émigré, and a counterrevolutionary, but he supported Napoleon in 1814–15.¹⁰ Also it is impossible to identify the number of royalists within the corps. In 1814 most of the prefects rallied to the Bourbons—Cobban's reason for declaring that previously they had been secret royalists—but often they changed sides only after the arrival of the Allied armies or the abdication of Napoleon. In 1815 some of them returned to Napoleon—were they secret royalists under Napoleon and secret Bonapartists under Louis XVIII? The few who in 1814 rallied to Louis before the collapse of the Napoleonic regime and subsequently followed Louis to Ghent were certainly royalists. Most prefects, however, made their decisions in 1814–15 on the basis of political calculation or loyalty to the administration and France, and not loyalty to Bourbon or Bonaparte. One example will illustrate how quickly loyalties could change and how meaningless some of these loyalties were. On June 25, 1814, a political *commissaire du roi* reported that the Napoleonic prefect Duval should be retained because he was an excellent administrator and “M. Duval is loyal; he is devoted to the royal government and he will remain faithful to it.”¹¹ A royalist undoubtedly, but he went back to Napoleon in 1815. Many of the royalists of 1814 could best be classed as *ex post facto* royalists—in applying to Louis XVIII for jobs they claimed to have been royalists all along, but if Bonaparte had survived they would have been equally emphatic about their loyalty to the Empire. For these reasons it has proven totally impossible to identify accurately the political émigrés or the royalists within the prefectural corps. They must remain what Cobban said they were—secret royalists.

“THE REVOLUTION IS OVER,” Napoleon declared in 1800. Then, to make sure that this was so, he set out to fuse the men of the Revolution, who were essentially bourgeois, with the men of the *ancien régime*, who were essentially noble. One method of breaking the class barriers of both Revolution and *ancien régime* was to appoint bourgeois and noble to the same institutions and make them work together. In social origins the prefectural corps was part noble, part bourgeois. This, of course, has always been known. But the important question has remained unexamined: what proportion was recruited from each class and how did these proportions change over

⁹ Donald Greer, *The Incidence of the Emigration during the French Revolution* (Cambridge, Mass., 1951).

¹⁰ F1b1 158–31.

¹¹ F1b1 158–41, fol. 21.

time. A number of historians have asserted that the proportion of nobles increased.¹² Some of them date this change from around 1801 or refer to an aristocratic reaction associated with the year of the Austrian marriage, 1810.¹³ The proportion did, in fact, increase, but the assertion is highly misleading, and the extent of the change has never been estimated.

Any investigation of social origins leads to the great debate as to what constituted the nobility and what was the bourgeoisie.¹⁴ Suffice it to say that there were no clear answers to these questions at the time and there are no clear answers now. Each historian is therefore obliged to define his own terms and to adopt his own methods of research. My sources show that 110 of the prefects were born into families that had been ennobled during the *ancien régime*.¹⁵ A further five years of exhaustive research might add a few more names but could hardly change the conclusions. Apparently none of the prefects was recruited from the lower class—that is, the peasants or the workers. Thus the remaining 171 were middle class (neither noble nor lower class), and the middle class may be defined for working purposes as the bourgeoisie. Some of these were clearly from the haute bourgeoisie, others from the petite bourgeoisie. It is impossible, however, to divide accurately all 171 prefects into two such loose categories, so they shall remain simply the bourgeois prefects.

Thus defined, an absolute majority of Napoleon's prefects, 171 or 61 per cent, were bourgeois in origin, a significant minority, 110 or 39 per cent, were noble. The great nobles of the *ancien régime* began to rally to Napoleon immediately after the coup d'état. In the first years of the Consulate Napoleon appointed as prefects a Larochevoucauld, a Chauvelin, and Claude Brugière de Barante. Such names as Brunateau de Sainte Suzanne, Ville-neuve-Bargemont, and Barral de Rochinaud appeared around 1805. In the middle period one sees a Houdetot and a Latour du Pin entering the corps. After 1811 the families de Rambuteau and de Sainte Aulaire gain their representation, and how could Napoleon ignore someone with a name like Scipion Cyprien Jules Louis Martin Marie Elizabeth, Marquis de Nicolai? The increase in the proportion of nobles and the compensating decline in the proportion of bourgeois was a gradual one, as table 2 indicates (January 1 of every year and March 1800).

The number of bourgeois was constant; they were in a majority in the

¹² For example, N. Hampson, *The First European Revolution* (London, 1969); and Godechot, *Institutions*, 588.

¹³ Cobban, *France*, 26; and Savant, *Préfets*, 170.

¹⁴ N. Richardson, *The French Prefectural Corps 1814-1830* (Cambridge, 1966), 179-244; C. B. E. Behrens, *The Ancien Régime* (London, 1967); and Alfred Cobban, *The Social Interpretation of the French Revolution* (Cambridge, 1964).

¹⁵ The principal genealogical dictionaries consulted to establish a list of nobles (in addition to other dictionaries and sources) include Chais d'Est Ange, *Dictionnaire des familles françaises anciennes ou notables à la fin du XIX^e siècle* (Evreux, 1903-29); Henri Jouglu de Morenas, *Grand Armorial de France* (Paris, 1934-52); A. Révérend, *Armorial du premier Empire* (Paris, 1894-97); and A. Révérend, *Les familles titrées et anoblies au XIX^e siècle* (Paris, 1901-06).

corps from first to last. The proportion of nobles increased because they received the positions created by the expansion of the Empire and not because they replaced bourgeois prefects. The real influx of nobles occurred in the first seven years of the regime. From 1807 to 1814 the proportion of nobles increased by only five per cent, an insignificant change that makes nonsense of the assertion that nobles were flocking to the corps in 1808 or 1810 or 1812. The aristocratic reaction, if ever there was one, occurred under the Consulate. What is true is that the nobles came to occupy a disproportionate number of the more important departments. In 1801 a larger proportion of bourgeois than of nobles were in the first-, second-, and third-class depart-

TABLE 2. SOCIAL BACKGROUND: NUMBER AND PROPORTION OF BOURGEOIS AND NOBLE PREFECTS

<i>Years</i>	<i>Number of Bourgeois</i>	<i>Proportion of Bourgeois</i>	<i>Number of Nobles</i>	<i>Proportion of Nobles</i>
1800	77	77%	23	23%
1801	74	75%	25	25%
1802	78	72%	30	28%
1803	74	69%	33	31%
1804	74	68%	35	32%
1805	76	70%	32	30%
1806	71	66%	37	34%
1807	69	63%	41	37%
1808	67	61%	43	39%
1809	73	64%	41	36%
1810	67	58%	48	42%
1811	74	59%	52	41%
1812	73	57%	54	43%
1813	74	58%	52	41%
1814	71	57%	53	43%

ments; by 1814 over half of the nobles occupied prefectures of the first three ranks. And, in 1812, the Seine or Paris passed from bourgeois to noble hands.

This proportion and concentration of noble prefects are sufficient to raise another question about the Napoleonic regime. Eugene Tarlé, the Soviet-Marxist historian, flatly states that the Napoleonic regime functioned in the interests of the bourgeoisie,¹⁶ an idea accepted by various historians, including George Lefebvre. A definitive conclusion would require exhaustive study of the policies followed by the individual prefects, but it would be curious indeed to discover that the 110 nobles administered their departments exclusively or even mainly in the interests of the bourgeoisie. What is more logical, and what conforms to the evidence, is that the attempt to impose a class-conflict theory on the Napoleonic regime, or at least on the administration of that regime, is fallacious, misleading, and pointless. What is more likely

¹⁶ Eugene Tarlé, *Napoléon* (3d ed.; Moscow, 1937), 6-7, 9, 110-11, 224, 319-22.

is that the Revolution was indeed over and that Napoleon's attempt to fuse the bourgeoisie of the Revolution with the nobility of the *ancien régime* was largely successful. Within the prefectoral corps one finds noble and bourgeois working together and working with ministers, subprefects, advisory councils, and the people of all classes. One finds a bourgeois prefect such as Antoine Thibadeau recommending a noble subprefect such as Arbaud Joucques for promotion.¹⁷ There is scarcely any evidence of class conflict or of real class interest. Bourgeois and noble prefects worked together in relative harmony. They appear to have administered France in the interests of all Frenchmen and, of course, in the interests of Napoleon Bonaparte. The proportion of nobles increased, but this occurred mainly in the first years of the regime. The bourgeois remained in a majority in the corps until the Restoration when the real aristocratic reaction began.

The prefects, it is often said, came from a wide variety of backgrounds. We are told that they were recruited from the Revolutionary assemblies, the diplomatic service, the Court, the army, the lower administration, and even from the Church. These generalizations suffer from four major weaknesses. They usually refer to the first hundred prefects and not the entire corps of 281. They do not tell us how many came from each background, which is, after all, the important question. They often confuse political, social, and functional backgrounds, as does Pierre Henry when he identifies the Revolutionaries (political background); six old aristocrats (social background); and three ambassadors, seven generals, and two administrators (functional background).¹⁸ Finally, the information itself is often erroneous. A brief acquaintance with the corps reveals that the prefects were drawn from four main occupations—the administration, the army, the legal profession, and politics. Those who worked in a single profession before becoming prefects present no problems of classification. Unfortunately most prefects had flirted with several occupations so that it becomes difficult to categorize their functional background. Charles d'Arberg, for example, was an auditor in the administration, a major in the army, a chamberlain in the Imperial Court, and had undertaken various diplomatic missions.¹⁹ Charles Delacroix had been an *avocat*, administrator of a department, politician, ambassador, and minister.

Given these problems, it becomes necessary to define exactly what is meant by functional background. For the purposes of this article, the term signifies the chief occupation, business, profession, or activity before becoming a prefect, or any occupation in which the prefect was engaged for more than five years. Someone who had been a lawyer for a dozen years and then entered politics for a few years and the army for a few more will be defined as a lawyer. On the other hand, an *ancien régime* army officer who sat in the

¹⁷ G. St. Yves and J. Fournier, *Le Département des Bouches-du-Rhône* (Paris, 1899), 60–61.

¹⁸ Pierre Henry, *Histoire des Préfets* (Paris, 1950), 22.

¹⁹ F1b1 155–6.

Convention and the Council of Ancients and was six years a subprefect will be classified as having military, political, and administrative backgrounds.²⁰ Sometimes the definition appears arbitrary: Joseph Mounier achieved his fame in two years in the National Assembly, but that short political interlude does not make politics his functional background. There is a very clear difference, after all, between political and functional background. Politics is only defined as a functional background if the prefect had spent more than five years in the assemblies or if most of his time had been spent in politics. By this standard the vast majority of the Revolutionary politicians are properly classified as having functional backgrounds other than politics.

The largest group within the prefectural corps—99 or 32 per cent—was recruited from the administration itself, and not from politics as most historians suggest. These men were the subprefects and secretaries-general, the intendants of the conquered provinces (there were no intendants from *ancien régime* France), the bureaucrats of local or national government, or the members of the diplomatic or consular services. The second most important functional background was law, from which 61 (20 per cent) of the prefects were recruited. There were a variety of titles for lawyers, including *avocat*, *parlementaire*, *notaire*, *procureur*, *juge*, *commissaire*, or simply *homme de loi*. Almost all of these men were lawyers before the Revolution, and most fulfilled legal functions during the Revolution. Many of them were elected to the assemblies, some were involved in local administration, but their chief occupation was law. Politics forms the third functional background. Forty-one (14 per cent) of the prefects had spent over five years in the assemblies or had spent sufficient time in politics for it to count as their chief former activity. The armed forces provided 36 (12 per cent) of the prefects. Most of these *militaires* were officers from the *ancien régime*, some of whom were elected to the assemblies. We are left with 63 prefects (22 per cent) of miscellaneous background. Their backgrounds are usually known, but their numbers are too few to consider separately. They include, for example, six chamberlains from the Imperial Court, four members of the clergy, perhaps a dozen businessmen, six academics, and a few landowners. Most of this group spent a few years in a variety of functions so that it is more accurate to classify them as miscellaneous than by any of the major categories. Finally, this category includes perhaps a dozen about whom nothing is known.

In so identifying the prefects we have been considering the corps as a whole, but when did these groups serve in the fifteen-year period? One in five was a lawyer, but when were the lawyers employed, and did their influence within the corps grow or diminish? Table 3 indicates the changing proportion of prefects in terms of background. The proportion of lawyers

²⁰ One prefect could therefore have as many as three functional backgrounds, so the following statistics are based on 309 identified backgrounds and not 281 prefects.

TABLE 3. FUNCTIONAL BACKGROUND

Years	Administration		Politics		Army		Law		Miscellaneous	
	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent	Number	Per cent
1800	19	17	26	23	8	7	35	30	27	24
1801	20	17	25	21	10	9	35	30	28	24
1802	19	15	24	20	13	11	37	30	30	24
1803	20	16	22	18	17	14	41	33	25	20
1804	21	17	22	18	19	15	38	30	25	20
1805	22	18	21	17	17	14	38	31	25	20
1806	24	19	22	18	17	14	38	31	23	19
1807	29	23	21	16	19	15	38	30	21	16
1808	30	23	22	17	19	15	38	29	21	16
1809	34	26	18	14	19	14	35	27	26	17
1810	39	28	19	14	17	12	39	28	25	18
1811	59	41	18	13	12	8	30	20	24	17
1812	59	41	18	12	14	10	29	20	25	17
1813	57	41	17	12	15	11	26	19	24	17
1814	65	47	15	11	12	9	22	16	23	17

declined steadily from one-third to one-sixth, the proportion of politicians from one-fifth to one-tenth. The number of *militaires* remained constant at around 10 per cent; but the number who were recruited from the administration rises from a relatively insignificant 15 per cent to almost half by 1814. What occurred was a gradual professionalization of the corps. In 1800 a majority were former lawyers and politicians; by 1814 nearly a majority had been administrators. The greatest change occurred in 1810 when the proportion of administrators jumped from 28 to 41 per cent. Curiously enough, this is the date some historians identify as the beginning of the decline in the quality of the administration.

IT WAS THE GOVERNMENT'S POLICY that prefects, unlike subprefects and secretaries-general, would never be appointed to the departments of their birth. This has long been known and, since it was official policy, has been assumed to be true.²¹ In fact, ten prefects were appointed to the departments of their birth; but the important questions remain: from which departments were they recruited and to which departments were they appointed? To determine geographical origins I have somewhat arbitrarily divided France into six regions, plus Paris. The 11 departments of the West (north of the Loire and west of 0° longitude) provided 26 prefects (2.5 per department). The 18 departments of the South (from the Isère to the Cantal to the Pyrénées Orientales) provided the same number per department, or 42. The area of least representation was the South-West (Charente Inférieure to Creuse to Ariège), where 27 prefects were born in the 16 departments.

²¹ Aulard, *Études*, 123.

Twenty-six prefects came from the eight departments of the North (Ardennes to Seine Inférieure) and 18 from the seven departments of the North-East (Meuse to Doubs), these areas sending more than three prefects per department. The rest of France, the 25 departments in the center, sent 58 prefects, and a further 37 were recruited in Paris. Thus Paris and the North proved the best recruiting ground for prefects.

No fewer than 31 prefects (12 per cent) came from outside the borders of France of 1789.²² They included thirteen Italians, nine Belgians, six Dutchmen, one German, and two Swiss. The number of foreigners was around seven from 1800 to 1810, then jumped to around sixteen (13 per cent) after that date. This is a considerable group of foreigners, but by 1811 the Empire contained 130 departments, one-third of which were non-French. The presence of this substantial group of foreigners forces a modification of another widely held idea about the Empire: by expanding so rapidly the French administration became overextended.²³ Such a generalization is true up to a point but must be qualified. In all the new areas Napoleon recruited promising people for the imperial administration, including even the Council of State. In 1812, for example, one in four ministers in the diplomatic service was non-French. By themselves the French were administering an area much larger than pre-Revolutionary France, but they were assisted by a large and growing number of people from these newly acquired territories.

While we know that prefects were rarely appointed to the departments of their birth, it remains to be seen whether there was a connection between their place of birth and their appointments. In fact, 22 per cent of the appointments were to departments in the neighborhood of their birthplace (within 100 kilometers), and half of these were to departments bordering on the departments where they were born. A further 17 per cent were to departments in the same area (within 200 kilometers), and 14 per cent were to departments within the same region (within 300 kilometers). Together with those appointed to the department of their birth, these appointments within the region of birth constitute an absolute majority of the known appointments, and four appointments in ten were to departments within 200 kilometers. These trends are even more pronounced for the non-French prefects. One-third of them served within 100 kilometers of the place of their birth, 14 per cent within 200 kilometers, and another 14 per cent within 300 kilometers, almost two-thirds serving within the area of their birth. In other words, the prefects in the Italian departments were often Italians, those in Belgium were Belgians, and most prefects served in the regions of their birth.

It has long been known that the prefects were young, but the assertion

²² Yet Godechot claims that "none came from the newly-acquired territories." Godechot, *Institutions*, 588.

²³ Geoffroy Bruun, *Europe and the French Imperium* (New York, 1938), 66-67.

has hardly been proven, and the exact meaning of "young" has never been identified. Pierre Henry, for example, states that 25 prefects were appointed in their twenties, which leaves us in ignorance of the age of appointment of the remaining 90 per cent of the corps and of the age of all the prefects while they held office.²⁴ The corps was indeed relatively young, especially in terms of the bureaucracies of the nineteenth and twentieth centuries. Twenty-three prefects (8.5 per cent) were appointed in their twenties, and the largest group—97 or 35.8 per cent—entered in their thirties. Ninety-six (35.4 per cent) were between 40 and 50. Only 55 (20 per cent) were over 50 at the time of appointment, with 11 of these in their sixties. As the regime went on, more and more young men were recruited from the lower levels of the administration, and the age of the prefects at the time of appointment

TABLE 4. AVERAGE AGE OF THE PREFECTS

<i>Years</i>	<i>Average Age</i>
1800	41.6
1801	43.0
1802	43.3
1803	43.8
1804	44.3
1805	45.4
1806	46.6
1807	46.8
1808	48.2
1809	48.1
1810	48.0
1811	45.5
1812	46.2
1813	47.0
1814	45.9

decreased. These figures reveal that the prefects were young at the time of appointment but tell us nothing about their age while in office. Table 4 gives the average age of the prefects as of January 1 of each year.

There was, as could be expected, a gradual aging of the corps caused by the retention of early appointees and the establishment of some degree of stability. The periods of instability or turnover, the *mouvements*, are readily identified—1800 when many of the first appointees were replaced by older men (shown on January 1, 1801), 1810, and 1813 when there occurred massive retirements of older prefects and their replacement by younger men. These two changes reflect the international rather than the internal situation. In 1810 Napoleon began drifting into war with Russia and had to reinvigorate his administration. It is commonly argued that a chief factor in

²⁴ Henry, *Préfets*, 44.

Napoleon's eventual defeat was the war weariness of the generals and the nation at large. Obviously Napoleon was perfectly aware of the problem. In 1813 there were major retirements of old and tired prefects or of any thought to be less than zealous in their administration in general or in the recruitment of troops in particular. They were invariably replaced by auditors from the Council of State or by subprefects, men who were young, ambitious, aggressive, had served no other regime, and were already experienced in administration. As a whole, the corps only aged 4.3 years during the fifteen-year period of its existence, which proves that there was a steady turnover of administrators.

A brief examination of the futures of the prefects will cast some light on their role as prefects. In particular, it will reveal whether the prefectural corps was a career in itself, the climax to a career, or a step to further promotion. One hundred and thirty prefects were still in office when the regime collapsed. What happened to them after the Allied invasion or after Napoleon's abdication is a problem of Restoration politics, which has been exhaustively studied by Nicolas Richardson.²⁵ Of the remainder, 19 prefects were promoted to the Council of State, the supreme legislative body in France. They included Jean Pierre Bachasson de Montalivet who soon became minister of the interior in charge of his former colleagues. These 19 men received a considerable proportion of the vacancies in the Council of State, which shows that Napoleon was using the prefectural corps as an apprenticeship for the highest positions in government. The prefects still in office in 1814 and those promoted to the Council of State constituted more than half of those employed by Napoleon. Another 21 died in office, and 18 have left little or no trace of their subsequent careers.

The remaining prefects are more difficult to classify. They were retired, transferred, or dismissed, but it is often difficult to distinguish between these three types of removal. Nineteen went to the Napoleonic assemblies, ten of these to the Senate. Twelve more retired without such honors, and two resigned. Twenty-nine were transferred to other administrative functions, and three went back to the army. Several became intendants of the conquered territories. A number entered the government in Paris, receiving such positions as *directeur général des mines*, *inspecteur général de l'instruction publique*, or *secrétaire général du Ministère de Police*. Laroche-foucauld entered the diplomatic service, three others became consuls. Many also went back to legal positions. On the whole it is probable that less than half of these transfers were to positions of equal or superior importance.

Twenty-eight prefects were dismissed, including those *destitué de ses fonctions* and those *appelé aux autres fonctions* but never reappointed. The motives, however diverse, were not often known to the prefects and are not always known today. Joseph Lagarde was suspended and ordered

²⁵ Richardson, *Prefectural Corps*, chs. 3 and 4.

to leave his department immediately, but he never found out why.²⁶ Felix Desportes, on hearing of his dismissal, wrote to the minister begging for an explanation and for an interview with Napoleon: "I am innocent of all the wrongs of which I am accused." But an interview would have accomplished little, as his successor reported that his department was in a state of complete confusion.²⁷ Claude Brugière de Barante was recalled in 1810 for his kindnesses towards Mme. de Staël and other former exiles, a piece of evidence that fits poorly into the general theory of an aristocratic reaction in that year.²⁸ Another noble, George de Belloc, was summarily dismissed in 1804 without ever receiving an explanation. Later on he discovered the cause. His wife, the daughter of the Duc d'Enghien's equerry, had expressed her horror at the execution of the duc. Her comments were in a letter to a friend, but the minister of police, Fouché, had all the friends of the Duc d'Enghien under surveillance. The letter was found on a suspect, and de Belloc found himself in premature retirement with his somewhat indiscreet wife.²⁹ Another royalist was cashiered in 1801 on the basis of a massive petition that accused him of being *chef de parti*, of juggling electoral lists, and of persecuting former revolutionaries.³⁰ Other prefects were dismissed for failing in various administrative tasks, failing to respond to crises, or failing to provide firm administration. Many were dismissed on the basis of petitions, reports from generals, the minister of war or of police, or the director of conscription. In the major turnovers of 1810 and 1813 the charge was usually failure to fulfill conscription quotas, the most serious administrative failing in any country facing imminent invasion.

Among the prefects there is one group whose future is a subject of intense interest to political historians of the period. These are the Revolutionary politicians. Lefebvre talks of the way Napoleon replaced his best administrators with second-rate men. In the Côte d'Or, he writes, the department passed through the hands of a Revolutionary, a tribune, a noble, and finally a duke. "The remaining survivors of the Revolution were few and far between."³¹ Lefebvre clearly means to say that Napoleon deliberately replaced the excellent Revolutionary politicians with inferior people. The idea that the Revolutionary politicians were independent-minded advisers capable of standing up to Napoleon pervades the historiography of the subject. Equally pervasive is the idea that Napoleon became irritated by such men and deliberately removed them from his government. Let us reserve these allegations for examination later in the article in order to turn to the question of what happened to the 83 Revolutionary politicians Napoleon employed as prefects.

²⁶ F1b1 166-5.

²⁷ Felix Desportes to the minister of the interior, Dec. 13, 1813, F1b1 176-11 and fol. 52.

²⁸ F1b1 150.

²⁹ F1b1 156-15.

³⁰ F1b1 167-1, dossier Magnytôt.

³¹ George Lefebvre, *Napoleon* (London, 1969), 2: 161-62.

Twenty-four Revolutionary politicians (29 per cent) were still in office in 1814. Sixteen (19 per cent), including Jeanbon St. André and Charles Delacroix, died in office. A further nine (10 per cent) were promoted, eight of these to the Council of State. More might have entered the Council of State except that the cream of the Revolutionaries entered the Council in 1800, the Council was relatively small, and its personnel was exceptionally stable. The promotion of these nine prefects suggests that Revolutionary politicians continued to move upward in the administrative pyramid even though there were few opportunities for such promotion. A number of politicians were retired, Nicolas Harmand at the age of sixty-seven, Jean Marquis, Joseph Martin, and Louis Milet at the age of sixty. Six more retired in their fifties, two in their forties, many of these going to the Senate.

More difficult to explain are those prefects transferred to other functions. Benoît Najac and Jean Jollivet simply returned to the Council of State after organizing their departments; Beytz and Charles Dugua received equivalent administrative positions in Holland and San Domingo. Jacques Guinebaud, at sixty-three, entered the consular service. A further eight, mostly in their late forties, received appointments as *juges*, members of the *conseil des prises*, *secrétaire du conseil des arts et commerce*, *maître des comptes*, or *receveur particulier des finances*, and François de Saint-Horent was reduced to the rank of *conseiller général* at the age of forty-two. Most of these transfers were normal administrative changes or semiretirements of older administrators. A few of them might be classed as demotions, but none of them were outright dismissals.

All but seven of the 83 Revolutionary politicians have been accounted for, and the reasons for the dismissal of J. Harmand and François Delattre are unknown. Nicolas Frochot was the most important prefect (Seine-Paris) in France from 1800 until 1812, when he accepted the unproven rumor that Napoleon had been killed in Russia. This piece of political gullibility immediately cost him his job, especially as an example had to be made. Alexandre Guérin de Chateauneuf-Randon, prefect of the Alpes Maritimes until 1802, had a checkered career. He entered the Court of the *ancien régime* as a page to the king and later to the Comte d'Artois, then became an officer in Artois's regiment. Betraying this royalist past, Guérin became a Jacobin Terrorist. He voted for the death of the king he had once served, sat on the Committee of Public Safety, and was sent on many missions during the Terror. He ended his career by spending the years 1812 to 1817 in debtor's jail. Antoine Rabusson-Lamothe was dismissed from the Haute Loire in 1810. Later, in an application for a job under the First Restoration, he claimed that he had been dismissed "by the perfidious insinuations of Lacuée, then minister of conscription." To have failed Napoleon in conscription was an advantage under the Restoration—Rabusson admitted he had tried to modify the harshness of conscription and, anyway, had always

been a royalist!³² Jean Sabatier was dismissed “on the denunciation of several officials in the Department.”³³ Finally Edme Bailly was one of the many prefects removed in 1813 for failing to recruit sufficient troops. This event occurred two years after the minister of war had officially reported to Napoleon that Bailly was somewhat feeble and too willing to conciliate. His dismissal followed repeated complaints from the minister, although Bailly claimed that he had done everything possible.³⁴

One wonders what is left of Lefebvre’s allegation that Napoleon deliberately removed the excellent Revolutionary prefects from the corps? The answer is—very little indeed. In 1814 almost one-third were still in office, and the Revolutionaries accounted for one prefect in five. The largest group who left the corps were those who died in office. Thirteen retired in their fifties or sixties, well over the average age of the prefects. Fourteen were transferred, but few of these could be regarded as demotions. Only six were dismissed outright, several perhaps unjustly, several with good cause. But against this must be set the nine who were promoted to higher office. It is true that the number of Revolutionary politicians fell from 50 to 24, but this attrition was caused by death, retirement, promotion, normal administrative transfers, and the mathematical inevitability of aging. In fifteen years in power Napoleon dismissed, demoted, and disgraced thousands of people. They included Revolutionary politicians as well as every other group who served him—noble and bourgeois, Jacobin and emigré, even his own friends and family. It cannot be said that he deliberately weeded the Revolutionary politicians from the prefectural corps.

FRENCH, ENGLISH, AND AMERICAN HISTORIANS have argued that Napoleon’s government gradually deteriorated, and some of them have mentioned specifically the deterioration of the prefectural corps. The prefects governed France; therefore the quality of their administration is one of the most important historical problems of the period. The deterioration of the prefectural corps is such a widespread assumption that the historiography of it and the evidence for it must be reviewed at some length.

George Lefebvre has written the best French account of Napoleon. He talks of the ministry itself and argues that gradually “the great administrators” were replaced with “second-rate men” and nobles, “many of [whom] were lacking in ability.” “The recruitment of prefects underwent a much more perceptible change,” which is identified as the replacement of Revolutionary prefects by nobles with the result that “there can thus be no illusion

³² F1b1 172-1.

³³ F1b1 173-1.

³⁴ F1b1 156-2.

about the imperfections of local government."³⁵ Lefebvre's evidence is the replacement of a few Revolutionary politicians with a few nobles. In full agreement with Lefebvre, on this issue at least, is the late Professor Cobban. Cobban repeats the general statement about "the ablest men in France" being replaced by "flunkies." In the prefectural corps Napoleon appointed more and more nobles and emigrés who "introduced more easy-going methods . . . and leave the real work to the secretaries-general. Many were now secret royalists." The American historian Geoffroy Bruun would agree: "Bonaparte's first generation of officials, trained in the pragmatic school of the Revolution, formed a group unique in character and experience. His second generation . . . was by his own confession less capable and less enterprising." Again the administration deteriorated because fewer Revolutionaries were used. Jacques Godechot agrees with Cobban that the number of nobles increased and that they would not hesitate to betray Napoleon, though no betrayals are actually mentioned. Paul Gagnon refers to the loss of "many able men" and adds that "to face mounting problems, Napoleon relied on lesser men." Jean Savant argues the same: the appointment of a noble was like replacing a colonel with a lieutenant.³⁶

None of these general accounts has given any real evidence to prove that this group of 281 administrators gradually deteriorated. Perhaps this is understandable in general works; perhaps the evidence is found in the more specialized studies. A local study that draws conclusions about the general quality of the corps is that of L. Benaerts, who argues that the prefects after 1804 were not as good as those of the Consulate.³⁷ Benaerts's work, however, provides little basis for his argument; his narrative only extends to 1804, he has no comparison of administrators before and after 1804, and his only evidence of inferior administration is a quotation from a biased subprefect. And, even if his evidence were sound, which it is not, no one should pretend to generalize from the exceptional situation of Brittany to the whole of the French Empire. In a similar study of the Bouches-du-Rhône (1800 to 1810), St. Yves and Fournier came to similar conclusions.³⁸ According to them, the administrators before 1805 were largely Revolutionaries, while those after 1805 were largely nobles who were enemies of the Revolution, critics of its institutions, and royalists in 1814. Their evidence does not include the prefects who, from 1800 on, were Revolutionaries; the dividing line of 1805 is largely arbitrary; the conclusions about 1814 are unsupported because the narrative only extends to 1810; and the only noble subprefect from the Bouches-du-Rhône who

³⁵ Lefebvre, *Napoleon*, 2: 161-62. It should be noted that most historians who write surveys of Napoleon are experts on the French Revolution and not on Napoleon. Naturally, they often see the Napoleonic regime in relation to the Revolution and not as a separate historical entity.

³⁶ See Cobban, *History of France*, 25-26, 55; Bruun, *French Imperium*, 66; Godechot, *Institutions*, 588; Paul Gagnon, *France Since 1789* (New York, 1964), 77; and Savant, *Préfets*, 170-73.

³⁷ L. Benaerts, *Le Régime consulaire en Bretagne* (Paris, 1914), 357-59.

³⁸ St. Yves and Fournier, *Bouches-du-Rhône*, 398-99.

became a prefect was not then a royalist. Furthermore, conclusions about the Bouches-du-Rhône are inapplicable to the rest of France, because the area was a notable center of royalism, religious strife, local patriotism, and opposition to the centralization of the French Revolution.

A more recent study is that of J. P. Soulet on the Hautes Pyrénées under Napoleon. This department had three prefects. The first, Bernard Lannes, was dismissed in 1802 for incompetence.³⁹ The second, the Revolutionary politician Jean Chazal, provided excellent administration from 1802 until 1813 when, as a non-noble, he was caught up in the social transformation of that year.⁴⁰ Much later Soulet explains that Chazal had been transferred to another department, not dismissed, and that the reason was not his social origin but his rheumatism, which made it impossible for him to work and led to his complete retirement in early 1814.⁴¹ Chazal was replaced by a nobleman, Arbaud-Joucques, who had been a subprefect for six years. By Soulet's admission Arbaud was far better prepared for the position than the Revolutionary politician he replaced, and, by Soulet's evidence, he was every bit as good an administrator as Chazal.⁴² Arbaud did not go over to the Bourbons until two weeks after Napoleon's abdication, at which time he made no apologies for having loyally served Napoleon.⁴³ The evidence, then, is of a useless administrator being replaced by a competent Revolutionary politician replaced in turn by a better qualified and equally competent nobleman. However, so strong is the opinion that the administration deteriorated that Soulet, in spite of his own evidence, asserts that Arbaud was just another of those docile emigrés.⁴⁴

The evidence for decline is in some ways denied by Godechot in his preface to Soulet's book. He accepts that Lannes was "without doubt unsuited for administration" (which is the important issue in a book on administration) and stresses the things the three prefects supposedly had in common. They were, he says, intelligent, honest, conscientious, loyal, and shrewd; they understood the interests of the state and the problems of the department, and all applied the orders of the government. The information in the book indicates that Lannes did not rank with Chazal and Arbaud in these matters. Then, by going outside the book's evidence, Godechot subtly discredits the noble Arbaud by saying that he was "representative" of the *ancien régime* and of the Restoration and of the landowners who, unlike the sans culottes of 1792-93, failed to rally to the defense of France. (Chazal

³⁹ Soulet, *Hautes-Pyrénées*, 14, 16, 39, 45, 233.

⁴⁰ *Ibid.*, 193.

⁴¹ *Ibid.*, 234-35.

⁴² *Ibid.*, 195-96.

⁴³ *Ibid.*, 225.

⁴⁴ *Ibid.*, 195. The adjective "docile" is constantly used to discredit nobles, yet the nobles of the *ancien régime* were anything but docile. On the other hand, some of the Revolutionary politicians were and had been docile. However, in describing them, historians prefer the adjective "moderate," which is always seen in a favorable light. This is a case of heads the Revolutionaries win, tails the nobles lose.

was not a *sans culotte* and Arbaud did defend France in 1814.) This one modern and thorough study of a department indicates clearly that Napoleon's first prefect was incompetent and that there was little to choose between the next two except that by 1813 Chazal was old and sick and Arbaud was better trained for the position. Why, we may wonder, does Godechot find it necessary to promote Lannes by equating him with the other two and to discredit Arbaud on the basis of the actions of other people of whom he was not at all representative?

Other regional and individual studies prove little about the changing quality of the prefects. Charles Poitevin-Maissemy, the object of a study of the Pas-de-Calais, was still in the corps in 1814, so that any conclusion about his ability would apply to the entire period. The same is true of Jean de Bry, prefect until the Restoration. An exhaustive thesis on Jeanbon St. André, cited in several bibliographies of the prefects, covers his activities as politician but not as prefect, and, like de Bry, he was in office until the Allied invasion. A study of the Rhine departments is excellent administrative history, but it tells us little about the administrators. The biographer of Jacques Beugeot argues that Beugeot was an excellent prefect, an opinion obviously shared by Napoleon, who promoted him to the Council of State and made him a chief minister in the satellite kingdom of Westphalia. In another regional study, that of the Meurthe under the Consulate, Thiry tells us that the first prefect was an excellent administrator. This was the *conventionnel*, Jean Marquis, but around 1808 he applied for his retirement because of illness. The study says nothing of his replacement. In short, these regional and biographical studies tend to agree with the theory of deterioration, but in no way do they prove it, and much of the information provided actually contradicts it.⁴⁵

The overwhelming opinion of all of these historians is that the prefectural corps deteriorated. But have they really proven their opinion? Some historians mention one or two excellent prefects who left the administration around 1810, and perhaps a few nobles who were appointed. In a corps of 281 prefects can one or two such changes prove a general deterioration? The arguments put forward can be summarized as follows: the number of Revolutionary politicians declined, therefore the administration deteriorated because Revolutionary politicians always made good administrators. At the same time the number of nobles increased, therefore the administration deteriorated because nobles always made poor administrators, or were royalists, or were "ready" to be disloyal. Only a few examples are produced to back up these arguments. Furthermore, the assumption about the quality

⁴⁵ See J. Chavanon and G. St. Yves, *Le Pas-de-Calais de 1800 à 1810* (Paris, 1907); Léonce Pingaud, *Jean de Bry* (Paris, 1909); L. Levy-Schneider, *Le Conventionnel Jeanbon Saint André 1749-1813* (Thèse, Paris, 1901); P. Sagnac, *Le Rhin français pendant la Révolution et l'Empire* (Paris, 1917); E. Dejean, *Un Préfet du Consulat, Jacques-Claude Beugeot* (Paris, 1907); and J. C. Thiry, *Le Département de la Meurthe sous le Consulat* (Thèse, Nancy, 1957), 29-30.

of Revolutionary politicians is entirely political, and tells us nothing about their quality as administrators; the assumption about the weakness of the nobles is a social one and so does not bear upon administrative matters. The arguments are not based on evidence and turn out to be little more than prejudices.

What might prove more fruitful would be to identify a number of accepted characteristics of quality in bureaucracy and then determine the degree to which these criteria were met by the prefectural corps. For this purpose several assumptions are necessary: an experienced administrator, by and large, is likely to be better at his job than an inexperienced one; a trained administrator is probably better than an untrained one; an administrator who rises through the ranks by promotion is likely to be better than one who does not; and a stable administration is preferable to an unstable one. In short, we may argue that a stable group of professionals who know their business are probably better at that business than a group of novices, no matter how successful those novices were in some other activity. This is, indeed, the gist of what Max Weber tells us: experience, training, hierarchy, promotion, and stability are the distinguishing features of bureaucracy; they are the essential bases for the quality of modern bureaucracy and for its superiority over nonbureaucratic forms of organization.⁴⁶ The details have never been agreed on, but only an anarchist could deny the general importance of these characteristics. We may usefully assume that, in evaluating the quality of an administration, these criteria, though perhaps not final, have more historical and universal validity than the assumption that any Revolutionary politician will be a better administrator than any person of noble birth.

Probably the most important attribute of the successful bureaucrat is experience. In general, the experienced administrator can be distinguished from the inexperienced one in that the former knows the routine and the rules; he is thoroughly familiar with the functioning of the administrative machine; he has dealt with many of the problems or observed his colleagues dealing with them; he has learned some lessons from his own mistakes and perhaps from those of others; he has learned how to interpret or apply national laws to local circumstances. One might observe that occasionally a totally inexperienced man is far superior to a civil servant with years in office or that, from time to time, a group of novices has worked miracles in administration. But such exceptions do not seriously contradict the rule, even if they attract the historian's attention. If this can be accepted, then one can generalize to the bureaucracy as a whole and assume that an experi-

⁴⁶ For administrative studies commenting on the characteristics of bureaucracy see C. Friedrich, "Some Observations on Weber's Analysis of Bureaucracy," in Robert K. Merton *et al.*, *Reader in Bureaucracy* (Glencoe, Ill., 1952), 29; W. A. Robson, ed., *The Civil Service in Britain and France* (London, 1956), 1; F. M. Marx, *The Administrative State* (Chicago, 1957), 22-51; F. M. Marx, ed., *Elements of Public Administration* (2d ed.; Englewood Cliffs, 1959), 34-37; and Max Weber, *Essays in Sociology*, ed. H. H. Gerth and C. Wright Mills (New York, 1946), 196-203.

enced bureaucracy is likely to be superior to an inexperienced one, and that the more experience that bureaucracy has, the better the government it is likely to provide. Experience is defined here as years spent in administrative work, including work in local or national government, in the diplomatic and consular service, or in the auditoriat of the Council of State. It has already been established that around one-third of Napoleon's prefects were recruited from other branches of the administration, thereby ensuring that many prefects were experienced bureaucrats when they arrived in the departments. Given the stability of the corps, a general rise in average experience was inevitable; but the details of that increase provide some interest.

The overall increase is indeed impressive—the prefects of 1814 had four to five times more experience in administration than those of 1800. The rise is almost perfectly gradual, with only slight declines in 1811 and 1814. By January 1, 1801, the average had climbed by more than one year, indicating that in 1800 Napoleon had dismissed a number of the first prefects and replaced them with more experienced men. This is a reflection of the haste with which the original list of prefects was prepared. The same occurred in 1804. In the eight years before 1810 the average rose by almost six years, reflecting the high degree of stability of that period.

The effects of the renewals of 1810 and 1813 are of particular interest. In 1810, seventeen per cent of the prefects were dismissed, and the compensating appointments caused the average age of the corps to drop by 2½ years. In 1813 nearly one prefect in five was dismissed, the average age of the prefects declining by more than one year. In both cases the average amount of experience of the corps declined only slightly. How could average age

TABLE 5. AVERAGE YEARS OF ADMINISTRATIVE
EXPERIENCE OF THE PREFECTS

<i>Years</i>	<i>Average Years of Administrative Experience</i>
1800	2.4
1801	3.5
1802	3.9
1803	4.2
1804	5.4
1805	6.5
1806	7.0
1807	7.8
1808	8.7
1809	8.9
1810	9.8
1811	9.5
1812	10.0
1813	11.5
1814	11.3

decline by $2\frac{1}{2}$ years and average experience by only four months? The answer is simple. In both years a large number of older administrators were replaced by much younger men who had been in the administration almost as long as the men they replaced. This fact casts an entirely new light on the famous "movements" of 1810 and 1813. They have often been seen as political purges or part of an "aristocratic reaction" in which able Revolutionary politicians were replaced by lazy *ancien régime* nobles. In 1810 the number of Revolutionary politicians did indeed decline, but only seven of the nineteen dismissed were Revolutionaries. On the other hand the number of nobles increased by only four, but their proportion remained the same owing to the expansion of the Empire. Obviously, what occurred in 1810 and in 1813 was neither a purge of Revolutionaries nor an aristocratic reaction: it was a significant administrative change in which a number of aging prefects were replaced by younger men fully qualified for promotion by way of apprenticeship. For this reason the average amount of experience declined only slightly during these two periods of massive renewal. If experience be the most important qualification for the administrator, then in this respect Napoleon's prefectural corps improved steadily from the beginning to the end, with the prefects of 1814 having four times as much experience as their colleagues of 1800.

STABILITY OF PERSONNEL may be readily identified as one of the main characteristics of a developed and professional bureaucracy. If half the bureaucrats are being replaced each year then few people know their jobs, few have experience, the better ones will try to find more stable employment, and the administration will be chaotic. On the other hand, if the bureaucrat can hold his job for a sufficient length of time then he can acquire wisdom in the execution of his tasks, accumulate the necessary information and knowledge, learn of other positions so that he may competently respond to a promotion or transfer, and fulfill his functions with some confidence and objectivity because he knows he has tenure. Of course, stability may lead to stagnation, with administrators who are too old, have occupied the same positions for too long, whose horizons have grown too narrow, who resist new ideas, and who cannot respond to new challenges. But that possibility will not blind the serious student of administration to the general truth that a degree of stability is highly desirable.

In 1913 Aulard stated that Napoleon's prefectural corps was stable, his evidence being that two prefects out of 281 served for more than fourteen years.⁴⁷ His generalization was true, though his evidence hardly proved it. The prefects remained in the corps for an average of $5\frac{1}{2}$ years and in each department for an average of 4.3 years, each prefect holding an average of

⁴⁷ Aulard, *Études*, 130.

1.3 appointments. Averages tell us little about individual records. Ninety-two of the prefects served less than three years, with 47 of these being appointed and dismissed within the first three years, clearly marking this period as one of the most unstable of the regime. Another 38 were appointed during the last three years, their careers under Napoleon being cut short by the fall of the regime.⁴⁸ Only seven of the 92 who served less than three years were appointed between 1803 and 1811. The second largest group, in terms of length of service, were the 73 prefects who occupied their positions for periods of three to six years. Forty-five served for periods of six to nine years; thirty-nine for nine to twelve. Above these, the real stability of the corps centered on the 33 prefects who served more than twelve years, many of them for the entire period. If one ignores the confused years of

TABLE 6. STABILITY: NUMBER AND PROPORTION
OF DISMISSALS PER YEAR

<i>Years</i>	<i>Number</i>	<i>Proportion</i>
1800	9	9%
1801	14	14%
1802	18	17%
1803	7	7%
1804	7	6%
1805	9	8%
1806	8	7%
1807	4	4%
1808	9	8%
1809	10	9%
1810	19	17%
1811	5	4%
1812	3	3%
1813	23	18%

the early Consulate when the administration was being established and the last year when it was disintegrating, one is left with a remarkably stable and permanent administration.

But the important question is whether the administration grew more or less stable. The best indicator of relative stability is the turnover per year, which can be established by comparing the number of dismissals per year to the total number of departments (see table 6). Clearly the period of greatest instability was from 1800 to 1802, with one-third of the prefects dismissed. This massive shuffle underlines the weakness of all those accounts based on the original appointments to the corps. From 1802 until 1809 the turnover was from six to eight per cent, which would be normal. The exces-

⁴⁸ We are concerned here with the stability of the corps under Napoleon and not with the individual careers. Many prefects still in office in January 1814 had long subsequent careers. A study of their entire careers would reveal a vast increase in the average length of service.

sive turnover of 1810 was followed by the two greatest years of stability of the period, so that the average turnover between 1810 and 1812 was seven per cent or normal. The attempt to head off impending disaster made 1813 the year of greatest change. Then, 1814–15 were years of massive instability in the prefectural corps.

Was this stability so marked as to make the corps stagnant? The answer is no. For one thing the Napoleonic period was really too short for administrators to have become stagnant. Bourgeois de Jessaint was a prefect from 1800 to 1838. By the 1830s he might have become somewhat set in his ways, but he certainly was not in 1814. Second, there was considerable transfer between departments. Third, the table of average age shows that the administration was turning over fairly well: it was becoming increasingly stable but remaining relatively young. So, after three years of uncertainty and confusion, the Napoleonic prefectural corps became remarkably stable and experienced only one major renewal before 1813. In terms of administration, this meant that after 1803 the prefects had the time, the opportunity, and the confidence to learn their jobs, familiarize themselves with their departments, and provide intelligent and stable administration.

One of the cardinal features of bureaucracy is a hierarchy or pyramid of positions running from least to most important. From the beginning the prefectural corps was organized in just such a fashion. At the top the Seine or Paris was in a class by itself, the prefect holding near-ministerial rank. The rest of the departments were divided into four ranks. In the first category were the six departments that contained the largest cities in the Empire, such as the Rhône with Lyons. In the second rank were the next 12 departments, such as the Nord with Lille. The third category contained 32 departments, the fourth, 79. The sixth rank consisted of the prefect's chief subordinate, the 130 secretaries-general, and beneath them were the 500 sub-prefects. In 1803 another level in the hierarchy was created. This was the auditoriat of the Council of State, some of whose members trained to be subprefects. From the auditor to the prefect of the Seine there were eight separate ranks or echelons in the prefectural hierarchy.

If hierarchy be the characteristic of modern bureaucracy, then promotion through that hierarchy is characteristic of the personnel. Promotion, in theory at least, is based on merit—the bureaucracy providing an opportunity for a career open to talent. Thus the vast majority of upper-level administrators, be they in government, the army, or business, have only arrived at the top after proving their ability during apprenticeship and earning promotion through the junior ranks. The reason for this is simply that it has long been recognized that the best administrators are those who work their way from the bottom or the middle to the top, learning about each level as they pass through it, being promoted to the higher offices only after the successful completion of more minor responsibilities, and being promoted on the recommendation of superior officials who identify the reasons for the promo-

tion. It follows that a measure of the quality of an administration is the degree to which the superior bureaucrats are recruited from the lower echelons of the administration. Again, one or two exceptions will not destroy the general validity of this rule.

An official report sent to Napoleon by Champagny, the minister of the interior, clearly shows that promotion through the ranks was indeed the policy of the Napoleonic government.⁴⁹ Champagny's list of candidates for important vacancies in the corps was composed of men who were already prefects: "It appeared to me that [Napoleon's] intention was that the honour of occupying the most prestigious positions in the career would be the reward for the zeal and talent which had been displayed in the less important positions." Of the eight prefects recommended five received their promotions within two years. There were many cases of promotion through the ranks. Alexandre Lameth, one of those recommended by Champagny, began in a fourth-class department, moved to a third-class one in 1806, then to one of the second-class in 1809. The career of Amable Brugière de Barante provides an almost perfect example of promotion through the hierarchy. After spending two years in local administration in Carcassonne, he entered the ministry of the interior as a student of administration. Later, as auditor of the Council of State, he fulfilled the duties of intendant of Danzig and by 1807 was subprefect. Two years later he was prefect of the fourth-class Vendée and by 1813 had been promoted to the second-class Loire-Inferieur. He was still only thirty-one, but it was his sixth promotion in thirteen years of administration.

The first method of determining the degree of promotion is to see what proportion of the total number of appointments in any year were, in fact, promotions. Table 7 indicates that in the first five years approximately 30 per cent of the appointments constituted promotions within the administration. Between 1804 and 1805 the proportion jumped from 23 per cent to 57 per cent and from then on averaged around two-thirds. So, during the Consulate one prefect in three arrived at that position as a result of promotion from lesser offices; after the establishment of the Empire twice as many prefects were promoted to office. A more accurate method of determining the degree of promotion is to establish how many prefects in each year had arrived at their positions as a result of promotion from inferior ranks. Table 8 shows that during the Consulate approximately 40 per cent of the prefects had been promoted, in the next five years around 50 per cent, and after 1810 over 60 per cent had arrived at their positions after apprenticeship at lower levels of the administration. The trend toward professional appointments from the junior levels was steady and consistent. If a good administration necessitates promotion through the ranks and apprenticeship at the junior levels before promotion to the senior, then the prefectural corps

⁴⁹ Champagny to Napoleon, Nov. 20, 1805, F1b1 150-152.

TABLE 7. NUMBER OF PREFECTS PROMOTED AND PERCENTAGE OF THE TOTAL NUMBER OF APPOINTMENTS THAT WERE PROMOTIONS

<i>Years</i>	<i>Number</i>	<i>Percentage</i>
1800	4 ¹	36%
1801	9	38%
1802	6	22%
1803	4	29%
1804	3	23%
1805	13	57%
1806	13	72%
1807	3	60%
1808	7	50%
1809	11	79%
1810	29	66%
1811	4	36%
1812	4	57%
1813	17	44%
1814	10	91%

was a relatively good administration that gradually and consistently improved throughout the period.

PROMOTION THROUGH THE RANKS is one method of recruiting able executives for the higher positions in a bureaucracy. Another method is specific training and education, the trainee entering the hierarchy at the junior officer or junior executive level and then being promoted upward toward the top. The two methods are complementary rather than contradictory. They ensure that the top of the hierarchy is staffed by a combination of two groups, one of which has worked its way from bottom to top and is rich in experience, the other of which has entered at the executive level and is rich in education and formal training.

This second method, that of formal education and training, has become increasingly popular due to the sophistication of modern society. I think it will be readily agreed that it is desirable for a man to be trained before entering high-level administrative functions. It would follow that a group of trained administrators could be expected to perform their tasks better than a group of untrained ones and that the more training an administration has, the better it probably is, providing that the recruitment of these trainees does not destroy the career open to talent for the other junior administrators. The system of training developed by Napoleon was the auditoriat of the Council of State, described by Markham as "one of the most interesting and original creations of the Napoleonic regime."⁵⁰

The auditoriat was devised as a common training program for all branches

⁵⁰ Markham, *Napoleon*, 98.

TABLE 8. NUMBER OF PREFECTS WHO ONCE HELD INFERIOR ADMINISTRATIVE POSITIONS AND THEIR PROPORTION OF THE WHOLE CORPS

<i>Years</i>	<i>Number</i>	<i>Percentage</i>
1800	41	36%
1801	49	40%
1802	51	40%
1803	47	40%
1804	46	40%
1805	52	45%
1806	59	50%
1807	59	51%
1808	69	57%
1809	72	58%
1810	90	64%
1811	82	62%
1812	81	61%
1813	92	63%
1814	88	63%

of the French administration. It began in 1803 when sixteen young men were attached to the five sections of the Council of State and the six ministries represented on the council. At the council the auditors listened to the debates, read reports, assisted the councillors, and undertook various administrative and educational missions. After several years of training the auditors were examined by three councillors of state upon whose recommendations they would be permanently attached to one branch of the administration or retained at the council for further training. It is impossible to deny the quality of the training these young men were receiving. To enter the auditoriat they had to have a university degree in public law. The opportunity to listen to debates in the Council of State, prepare reports for the councillors, go on fact-finding missions to all parts of France and of Europe, work directly with a minister, report directly to the council, and be interviewed by Napoleon himself would make today's administrative trainees envious indeed. Many of those who passed the examinations became subprefects, as one-quarter of the subprefectoral positions were reserved for auditors. Then, if successful, they could be promoted to the rank of prefect. There could not possibly be better preparation for a prefecture than the duties of subprefect or intendant of a conquered province.

Often the program was not followed. Young men were sometimes promoted before they were ready and some members of the administration were appointed auditors without receiving the training. But several careers will show clearly the type of training that some of these young men received before becoming prefects. Rouen des Mallets entered the auditoriat in 1809 and spent two years as intendant of Ragusa and Carniole before becoming

prefect in late 1813.⁵¹ After formal education in the École polytechnique and the École des ponts et chaussées, Camille Basset de Chateaubourg trained for two years in the auditoriat before becoming subprefect in 1806. Seven years later he was promoted to the prefecture of the Vendée.⁵² G. J. A. Stassart trained in the auditoriat for two years, spent 1806–07 as intendant of the Tyrol, Elbing, Königsberg, East Prussia, and then Berlin, and was subprefect for three years before becoming prefect of the Vaucluse.⁵³ The real influx of auditors began with the appointment of twelve men in 1810, eight more in 1813, and a further six in 1814. By 1813, 32 of the 130 prefects were auditors. In all, 42 auditors were promoted to prefect, 18 of whom had been subprefects. However, these 42 were just an anticipation of what would have come. With one-quarter of the subprefectures reserved for the auditors, Napoleon obviously intended eventually to recruit almost all prefects from the auditoriat by way of the subprefectures.

The prefects appointed in 1810–14 were the best-trained men ever to enter the corps under Napoleon—in fact they were possibly the best-trained prefects to enter the corps in the nineteenth century. Alas, the whole system was abolished in 1814. The reason, sad to say, was the rise of representative democracy. In France regular elections began under the Restoration. Almost the first consequence was that the prefects were told to deliver the vote to the government candidates. A change of government, therefore, meant a change of prefects. In this way the chief connection of the prefectural corps came to be with the government and the Assembly and not with the subordinate levels of the administration. Siwek-Pouydesseau reports that from 1876 to the Vichy regime only a small fraction of subprefects was recruited from the administration. “Before 1928, no test of ability was demanded for entrance into the sub prefectural corps. Political patronage was the only means of obtaining these positions.”⁵⁴ Since the chief activity of the prefects was political, a system of administrative training was pointless. It was only in the 1920s and 1930s that the French administration began developing a system of formal training and promotion for the prefects reminiscent of the auditoriat.⁵⁵ The British civil service did not adopt similar standards until the mid-nineteenth century.⁵⁶ Napoleon’s system, although faulty in its application and subject to abuses, was already well developed by 1810 and was providing an increasing proportion of prefects by 1812. Markham claims that with the auditoriat “Napoleon was at least fifty years ahead of his time

⁵¹ F1b1 177–17.

⁵² F1b1 156–8.

⁵³ F1b1 173–20.

⁵⁴ Siwek-Pouydesseau, *Le Corps Préfectoral*, 35.

⁵⁵ For the reforms in the French prefectural corps and administration in the 1930s see W. A. Sharp, *The Government of the French Republic* (London, 1938), 156–57; and Siwek-Pouydesseau, *Le Corps Préfectoral*, 46 ff.

⁵⁶ Edward Bridges, “The Reforms of 1854 in Retrospect,” in Robson, *The Civil Service*.

in his conception of a trained higher civil service."⁵⁷ In terms of the prefectoral corps, he appears to have been closer to a century ahead of his time.

The quality of Napoleon's prefectoral corps is reflected in one professional source of information that is especially interesting because it has not been seriously considered by historians. From time to time the minister of the interior prepared for Napoleon a resumé of his opinions on all the prefects. These assessments contained his opinions on each prefect's administrative ability, whether he was too old or too young, his relations with his *administrés* or with various groups in the department, whether he was respected, whether he was having difficulties, whether he should be promoted, transferred, retired, or dismissed. In one sense these opinions are subjective, the opinions of only one man toward his fellow administrators. On the other hand these assessments are highly professional, the reports of a minister on his immediate subordinates submitted to a ruler who knew many of the prefects personally, who had read many of their reports, and who would not likely be swayed by the minister's personal bias. It is on these professional reports that many prefects were promoted or dismissed, so they provide a useful commentary on the government's opinion of their quality. Unfortunately, they were not submitted regularly. And, naturally, different ministers held different opinions. Several have been published, especially those for 1800, 1812, 1813, and for the time of individual appointments.⁵⁸

The reports are far too extensive to analyze here in detail. But after reading through them it is apparent that most prefects were well qualified in the eyes of the administration. There is no evidence in these reports that the bureaucracy was deteriorating. Some poor prefects were singled out in the early period. Jean Huguet, for example, was "surrounded by enemies of the government," and Pierre Montaut-Desilles "had been accused of feebleness." In the latter period other prefects were found wanting. The auditor Jean Duval was "a little light." Joseph Frain had been in the corps since 1800, but by 1813 it was noted that "his administration is lifeless. . . . He is not a bad man, he is just a nobody."⁵⁹ Prefects thus described usually disappeared quickly from the administration. In the latter years the criticisms tended to be levied against the older prefects, precisely those people who are held out by some historians as being the best administrators. In previous reports they had proven satisfactory, but they were growing tired. The reports do not suggest any deterioration and, on the contrary, suggest that a number of inadequate appointees of the early years were eliminated and that the prefects who grew feeble were progressively retired. This evidence suggests an administration that was constantly being examined, constantly renewed, and gradually improving or remaining constant through the elimination of ineffective administrators.

⁵⁷ Markham, *Napoleon*, 99.

⁵⁸ Savant, *Prefets*, 218-321.

⁵⁹ *Ibid.*, 220, 271, 224, 225.

To sum up, the founding of the prefectural corps was one of the most lasting of Napoleon's achievements. Napoleon began with 97 men who were mainly bourgeois in social origin; who were usually in their thirties or forties; who were lawyers, politicians, and, to a lesser extent, administrators and soldiers; and who were usually appointed to a department in the region of their birth. As time went on the nature of the corps changed perceptibly. The bourgeois remained in the majority, but the corps came to be more evenly balanced after the early influx of the nobles. It became a successful example of the fusion of noble and bourgeois, of Revolution and *ancien régime*. Many of the lawyers and politicians retired, transferred, were promoted, or died. Their places were often taken by trained and experienced administrators. After several years of confusion and turnover the prefectural corps developed marked characteristics often identified as the features of modern bureaucracy, and indeed as the chief advantages of modern bureaucracy. The corps became stable, more professional, and more experienced; it became subject to fairly regular evaluation, with the advancement of careers based on promotion through the ranks; a formal system of training was acquired that was not re-established until the mid-twentieth century. It has long been argued that the prefectural corps, in common with the whole Napoleonic administration, deteriorated after the early years of excellence. This argument has been everywhere asserted and repeated but nowhere proven or documented. Much of the evidence adduced for it is fragmentary or contradictory. The basic argument for deterioration has been that "docile" nobles replaced "excellent" bourgeois Revolutionaries. This is to turn the *ancien régime* upside down with the bourgeois now entitled and qualified to rule by nature of their birth! It is an argument that appears to be founded on nothing more substantial than a prejudice. The application of more acceptable criteria for administrative quality reveals a Napoleonic prefectural corps that gradually and steadily improved throughout its fifteen-year existence.

The Cold War Warmed Over

A Review Article by WARREN F. KIMBALL

JOHN LEWIS GADDIS. *The United States and the Origins of the Cold War, 1941-1947*. (Contemporary American History Series.) New York: Columbia University Press. 1972. Pp. ix, 396. Cloth \$12.50, paper \$3.95.

JOYCE and GABRIEL KOLKO. *The Limits of Power: The World and United States Foreign Policy, 1945-1954*. New York: Harper and Row. 1972. Pp. xii, 820. Cloth \$15.00, paper \$6.25.

ROBERT JAMES MADDOX. *The New Left and the Origins of the Cold War*. Princeton: Princeton University Press. 1973. Pp. ix, 169. \$7.95.

IN GENERAL, cold war historiography has followed a pattern familiar to diplomatic historians. First come the "white paper" style histories, often written during a particular crisis and reflecting the view of government policy makers. Close on the heels of official and semiofficial apologies come the initial attempts at revision. Limited by the lack of evidence, these studies frequently spring from partisan politics and/or personal opposition to administration policy, and they are often characterized by visions of conspiracy. At this point defenses of official policy appear. Based on greater amounts of documentary evidence they often accept a few of the revisionist arguments while rejecting the revisionist's overall conclusions. These scholarly defenses bring on scholarly revisionism—though usually far more restrained in tone than the earlier critiques. Finally, time plus the desire to say something new results in an eclectic synthesis that, though based upon an examination of documentary materials, seems to approach the golden mean by extracting arguments and conclusions from all sides. Thereafter the last three steps are repeated again and again, though often rhetoric overpowers scholarship.

This general progression is not, of course, rigidly adhered to. Graduate

I wish to express my thanks to faculty members of the Rutgers-Newark history department seminar for their help. I am particularly grateful to my colleagues, Elliot Rosen and Herbert Meritt, as well as Professor Charles C. Alexander of Ohio University for having read and critiqued the entire manuscript.

professors all too frequently train rather than educate their students, and this results in the creation of long-lived schools. More importantly, external events often shape intellectual attitudes. Just as World War II affected the perspective from which most historians looked at World War I, so the Vietnam War brought forth from the left a strong and intense critique of American policy as it related to the origins of the cold war.

The three books under review, however, do fit the pattern fairly well. Joyce and Gabriel Kolko's *The Limits of Power*, which is a continuation of Gabriel Kolko's *The Politics of War*,¹ represents a left revisionist response to the scholarly defenses of official policy that dominated American diplomatic histories prior to the Vietnam War. Robert J. Maddox's attack on the work "of the most prominent New Left" historians fits in with the old orthodox defenses of American policy, even though Maddox is sometimes erroneously labelled a "younger scholar."² John L. Gaddis's *The United States and the Origins of the Cold War, 1941-1947* epitomizes the eclecticism of a new generation of scholars just beginning to make its mark.³

Although review articles are often intended as state-of-the-literature pieces, some of the routine of normal book reviewing is required, particularly in these three cases. Joyce and Gabriel Kolko posit a simplistic theme reminiscent of Arthur Schlesinger, jr.'s unsophisticated definition of liberalism as "ordinarily the movement on the part of the other sections of society to restrain the power of the business community."⁴ The Kolkos merely include the liberals as part and parcel of the business community and hence the villains of the piece: "As we shall see repeatedly, it is the expansive interests of American capitalism as an economy with specific structural needs that guide the definition of foreign economic policy and the United States' larger global role and needs."⁵ Since the goals of American policy are dictated by institutional necessity, they specifically reject the notion that American society can change its direction merely by electing good men. At the core of all American programs and policies during and

¹ Gabriel Kolko, *The Politics of War: The World and United States Foreign Policy, 1943-1945* (New York, 1968).

² Maddox was born in 1931 and attended the University of Wisconsin (Madison) and Rutgers University (New Brunswick) at the same time as many of the revisionists he critiques. The "younger historian" tag is attached by John L. Gaddis, "Checking History's Footnotes: *The New Left and the Origins of the Cold War*," *Book World*, May 6, 1973.

³ Other examples of eclectic scholarship are George C. Herring, Jr., *Aid to Russia, 1941-1946: Strategy, Diplomacy, and the Origins of the Cold War* (New York, 1973); Thomas M. Campbell, *Masquerade Peace: America's UN Policy, 1944-1945* (Tallahassee, 1973); both accept many of the left revisionist arguments while denying the ideological underpinnings. Herbert Feis's *From Trust to Terror: The Onset of the Cold War, 1945-1950* (New York, 1970), though admittedly written as a refutation of the left revisionist interpretation, is a classic if occasionally amusing example of creeping eclecticism. In spite of stridently antirevisionist conclusions and footnotes, Feis repeatedly agrees with many of the most important left revisionist arguments regarding economic diplomacy. Considering Feis's earlier historical efforts, such as *The Diplomacy of the Dollar, 1919-1932* (Baltimore, 1950), it is not surprising that such data seemed important—even if his conclusions often disregarded it.

⁴ Arthur Schlesinger, jr., *The Age of Jackson* (Boston, 1945), 505.

⁵ Kolko and Kolko, *The Limits of Power*, 8.

after World War II, the Kolkos feel, was a simple, carefully formulated objective—"to restructure the world so that American business could trade, operate, and profit without restrictions everywhere." Unfortunately American leaders failed to comprehend fully the political requirements of such a goal. The result was much more than a struggle with the Soviet Union for power—in fact the Kolkos find that battle only a convenience for American leaders—rather, it was a conscious attempt by the United States to crush the "Left" (the term is always capitalized by the Kolkos). The Kolkos find World War II merely a prelude to the "irreversible" changes in society that are presently occurring. Clearly this view reflects no "tragedy of American diplomacy"—with the implication of lost opportunities for change—but is instead the old Hegelian "world force," moving events irresistably, inevitably, and mechanistically toward their predetermined end.⁶ So much so, in fact, that the Kolkos' arguments begin to seem counter-revolutionary and to militate against the development of any radical consensus. Why construct a radical position when the triumph of the left is absolutely inevitable? A tragedy exists only after a person or society has made a ruinous choice—but the Kolkos merely chronicle the pre-ordained decline of the American nation.

In spite of such clear philosophical views the entire book suffers from the lack of coherent, consistent, usable definitions. Although the "Left" is defined by the Kolkos as the rising working class in Europe, it is also identified with anticolonialism in Asia and elsewhere. Combining as complex a group as the European working class with the Asian, African, and the Latin American peasantry is intellectually indigestible. Whether such confused concepts stem from intellectual or linguistic problems is difficult to determine, for one of the most frustrating features of the book is its turgid, repetitive, imprecise prose.⁷ For example, in deprecating the "cold war" as an unhelpful term, the authors assert that the "more significant context for understanding postwar history is the entire globe and the revolution, the counterrevolution, and the great, often violent, interaction between the United States, its European allies, and the vast social and economic transformation in the Third World *that is the defining fact of our world.*"⁸ I think I know what they are trying to say, but who can say for certain without precise instructions just what is modified by the italicized phrase. When "the defining fact of our world" is identified, we should have no doubt as to exactly what it is.

⁶ The above discussion is all taken from the introduction to *ibid.*, 1–8. Although I deal with *The Limits of Power* primarily as it relates to the debate over the origins of the cold war the book also deals with American foreign policy during the period of the institutionalization of the cold war—from 1947 through 1954.

⁷ Every student, without exception, in my recent seminar on cold war historiography at Rutgers University has agreed with the assessment of the book as unreadable. That includes a number of students who enthusiastically endorsed the Kolko's interpretations. I am indebted to those students for their stimulating analyses of the literature on the cold war. I am particularly grateful to Nancy Beck who will recognize many of the points she made in her seminar paper. The definitions are in Kolko and Kolko, *The Limits of Power*, 4.

⁸ *Ibid.*, 12 (italics added).

Similar examples abound. The role of public opinion in the late 1940s is briefly analyzed and dismissed because "over three-quarters of the nation simply paid no attention to foreign affairs and was apathetic or ignorant toward it." Yet a few sentences later American leaders are pictured as concerned with achieving their goals "over the indifference or opposition of the majority of the people."⁹ Do or do not the people care? This is not manipulation or distortion; it is simply poor writing. A good editor—one who would catch such contradictions and drastically cut the length of the book—would have been a blessing.

The rigidity with which the Kolkos apply their ideological framework results in some curious interpretations and undocumented conclusions. The chapter dealing with the outbreak of the Korean War is a good example. The sum of their arguments is that the Korean War was a logical and understandable response to the military build-up in South Korea; that American and South Korean intelligence experts knew of the North Korean build-up, but apparently said nothing; that an easy series of victories by North Korea served the interests of the South Korean president, Syngman Rhee, since they would eventually deeply and irrevocably involve American forces in the struggle; and that the whole crisis was part of some vague conspiracy designed to make American fears of a worldwide Communist plot a self-fulfilling prophecy. This entire presentation is based primarily on the basic philosophy underlying the entire book and the fact that military reports during the first days of the crisis were obscure and contradictory.¹⁰ If contradictory military reports during a surprise attack are proof of conspiracy then historians have much rewriting to do.

In one sense this is a most honest presentation, for the Kolkos lay out their ideological preconceptions for all to see. If you accept their notion that mankind cannot really affect its fate, and that one can therefore select any individual government official and present his or her views as those of "Washington," then their book is persuasive. What makes American readers so very uncomfortable is their method of isolating selfish, usually economic, motives behind the more hallowed episodes and aspects of American foreign policy. Thus the Marshall Plan, which was originally painted as sheer altruism and which later historians saw as part of an overall economic recovery plan for both Europe and America, becomes a quasi-conspiratorial scheme for the penetration of Europe by American capital. Intervention in Korea is no longer a defense against aggression, but instead a means of propping up a pro-American, right-wing tyrant. Anticolonialism, once thought a bulwark of traditional United States foreign policy, becomes merely a façade designed to hide the real American goal—the displacing of British colonialism by American imperialism. All this occurs because the Kolkos impose their own ideological orthodoxy upon their historical sub-

⁹ *Ibid.*, 333.

¹⁰ *Ibid.*, 574–85.

jects. In accepting an imperative determinist framework they apparently fail to understand that humans regularly act from a complex set of often contradictory motives; thus the Kolkos suffer from the same limited horizons as the liberal historians they argue against.

Even with these faults the book adds much to our knowledge. Their investigation of those motives they find important is thorough, and their evidence forces us to integrate the economic and social drives that underlay much of our foreign policy with the more traditional treatments of power and politics. As a bibliographical guide alone the book provides a definitive compilation of the primary sources available for studying American foreign policy from 1945 through 1954.

John L. Gaddis's *The United States and the Origins of the Cold War* has been hailed as "a successful response to the current revisionist fad."¹¹ Yet time and again Gaddis accepts many of the most significant left revisionist arguments. Granted, he puts them in a broader context, re-emphasizing politics, power, and domestic opinion, but throughout it all economics plays a primary role. Had Gaddis, six or seven years ago, presented the idea that "Washington chose to withhold the one instrument which might have influenced Soviet economic behavior—a postwar reconstruction loan—in hopes of extracting political concessions,"¹² his work would today be interpreted as an example of New Left history. The applause from some quarters for Gaddis's work as an answer to the left revisionists stems primarily from a tendency to lump such diverse historians as Gar Alperovitz, Lloyd Gardner, and Gabriel Kolko under one label, instead of evaluating each as an individual.

Gaddis's contribution is primarily his eclectic combining of the best of recent scholarship. He has used the documents extensively, but his interpretations show the cumulative effect of twenty-five years of historical writings on the cold war. He finds that the basic American goals during World War II began, logically enough, with a commitment to win the war and conquer the enemy. The next goal, that of achieving a permanent peace, fostered three specific policies: the promotion of self-determination, cooperation with the United Nations, and the prevention of future economic depressions—all worldwide problems. The last major objective, the creation of a cooperative relationship between the major powers, stemmed from the realization that such cooperation was necessary for the attainment of the other goals.¹³

Gaddis reconciles his orthodox and revisionist interpretations by placing Roosevelt's priorities in descending order of importance: military victory overshadowed all else, coalition diplomacy as it related to the wartime

¹¹ John W. Spanier in a review in *Journal of American History*, 59 (1972): 768-69.

¹² Gaddis, *Origins of the Cold War*, 23.

¹³ *Ibid.*, 2.

alliance came next, and postwar diplomacy came last.¹⁴ Like some revisionists, he finds great consistency between Roosevelt's and Truman's policies, though he seems to contradict himself when he agrees that Truman was strongly influenced by Harriman and Forrestal to move toward a hard-line approach.¹⁵ He makes sense out of the knotty problem of American policy in Eastern Europe by identifying two fundamentally contradictory goals: self-determination and the unity of the Grand Alliance. Since Gaddis earlier identified open channels of trade as a way Americans sought to promote peace, self-determination can be seen as a means of guaranteeing the existence of such open channels in Eastern Europe. Given the fact that by late 1943 Roosevelt had tacitly agreed to Soviet domination of Eastern Europe, any attempt to push for self-determination was bound to weaken or destroy Soviet-American trust.¹⁶

If there is one basic reason for the development of the cold war, Gaddis finds it in Roosevelt's refusal to correlate his private diplomacy with his public policies. Gaddis can accept the normal tensions that go with a world of nation states, but he finds the special nature of the cold war in America to be a product of Roosevelt's consistent pie-in-the-sky rhetoric about the future of international relations. Maybe the real difference between FDR and his successor was that Truman operated from the assumption that Roosevelt meant what he had said about demanding Soviet cooperation, while Roosevelt seems to have been engaged in but another campaign to educate public opinion.

Although Gaddis refrains from any further evaluation of Roosevelt's tactics in foreign policy, the mere recounting of the facts adds much to our knowledge of what Arthur Schlesinger, jr. recently called the "imperial presidency." To follow the twists and turns of something like American policy regarding postwar Poland—implying one thing to the Russians, another to the British, and still another to the American public—is to come to believe that the only thing that saved Roosevelt from the same loss of credibility as Lyndon Johnson is that FDR won his war.

The books by Gaddis and the Kolkos are both important, thought-provoking studies that offer positive theses and *ad rem* argumentation. While they often disagree totally on the interpretation given the same set of facts, each can be read and studied with great profit. The other book under review is essentially a historiographical critique and thus requires somewhat different treatment.

Robert J. Maddox, in *The New Left and the Origins of the Cold War*, raises a set of disturbing questions for our profession. Maddox examines portions of the work of "seven of the most prominent New Left" historians—William A. Williams, D. F. Fleming, Gar Alperovitz, David Horowitz,

¹⁴ This is true throughout the book, but see particularly *ibid.*, 63–94.

¹⁵ *Ibid.*, 215, 202–05, 230.

¹⁶ *Ibid.*, 21, 133–34.

Gabriel Kolko, Diane Shaver Clemens, Lloyd C. Gardner. He concludes that their "books *without exception* are based upon pervasive misusages of the source materials. . . . Even the best fails to attain the most flexible definition of scholarship. Stated briefly, the most striking characteristic of revisionist historiography has been the extent to which New Left authors have revised the evidence itself."¹⁷ Since Maddox assumes that these seven writers make up a representative selection of New Left historians his charge is of enormous import for it encompasses, by inference, a vast body of historical literature dealing with all aspects of American history. Because Professor Maddox's major thrust concerns the misinterpretation of documentary evidence it is perhaps best to let him speak for himself.

He begins by dividing the New Left revisionists into two groups: "The 'soft' revisionists [who] place far more emphasis upon individuals than they do on the nature of institutions or systems" and the "hard" revisionists who argue that "the Cold War was the inevitable result of the American system as it developed over the years." Since "most of the revisionists frankly proclaim that they conceive of their work as a tool for change," Maddox concludes that they believe "the historian's job, therefore, is to create a version of the past which can be used to help achieve the goals his ideological preferences dictate." But Maddox does not limit his criticism solely to the predilections and goals (as he sees them) of New Left historians. After citing comments by Williams and Horowitz, which set forth their reasons for writing an interpretation of American cold war policy, Maddox also infers a professional insult. Their "conclusion," he says, "is inescapable: only the revisionists possess sufficient courage to reveal truths which must have been obvious to even the most dull-witted orthodox scholar."¹⁸

In addition to accusations of misuse of evidence Maddox also criticizes the significance that revisionists attach to certain events. "Revisionists almost always employ a double-standard: Russia's actions are justified or explained by reference to national security or *Realpolitik*, Western actions are measured against some high ideal and found wanting." Maddox then gives a few brief examples of revisionist use of loaded words to misrepresent the facts, such as Kolko's "outrage" at the suppression of the left in Greece, compared to his downplaying of the Katyn Forest "murder," and the New Left view that "the United States invariably wishes to 'penetrate' Eastern Europe whereas the Soviet Union seeks merely 'economic partnership,' a semantic distinction Eastern Europeans might grimly enjoy."¹⁹

¹⁷ Maddox, *The New Left*, 10–11. Maddox discusses the following books: William A. Williams, *The Tragedy of American Diplomacy* (rev. ed.; New York, 1962); D. F. Fleming, *The Cold War and Its Origins*, 2 vols. (New York, 1961); Gar Alperovitz, *Atomic Diplomacy* (New York, 1965); David Horowitz, *The Free World Colossus* (New York, 1965); Diane Shaver Clemens, *Yalta* (New York, 1970); Lloyd C. Gardner, *Architects of Illusion* (New York, 1970); and Kolko, *The Politics of War*. I have referred to the edition Maddox cited most frequently, though he does make occasional references to other editions of those books.

¹⁸ Maddox, *The New Left*, 4–6.

¹⁹ *Ibid.*, 7–8. "Outrage" and "murder" are Maddox's terms.

After this tantalizing digression into the realm of differing interpretations (for surely the choice of emphasis is just that) Maddox returns to his basic argument: that New Left historians have distorted the evidence.

Before examining a few examples of Maddox's specific charges some general comments on his overall approach are in order. First, and most weighty, Maddox clearly implies that distortion of the historical record by the New Left was conscious and with malice aforethought. Although Maddox carefully avoids such a clear and unequivocal accusation the reader can entertain no doubts as to what he believes. At a recent meeting of the Columbia University American Studies Seminar (October 18, 1973) Maddox refused to affirm or deny such a view. His examination of the evidence, however, follows a discourse clearly criticizing the revisionist concept that history is a tool for change. Taken within the context of the book, that obviously implies that New Left historians care little about truth and hence would alter the evidence to suit their political purposes. Maddox comes close to leveling squarely the accusation in his closing sentence: "Perhaps, after all, the New Left view of American foreign policy during and immediately after World War II can *only* be sustained by doing violence to the historical record."²⁰ In a nutshell, Maddox has impugned the ethics and honesty of seven people by name and many others by implication. Those are not charges that can be lightly dismissed, nor can they be defended on the principle of the "greater good"—that is, by criticizing Maddox's vindictiveness, but then praising him for bringing about higher standards of scholarship. That sort of logic smacks too much of the defenses (made in the name of national security) of Senator Joseph McCarthy's actions, or of burning down school buildings in the name of education.

Every charge and specification leveled by Maddox cannot and should not be reviewed in this article since Maddox's method is to check footnotes and then determine whether or not the author misused the document.²¹ He is particularly sensitive to partial quotations that he claims were used in order to fit the evidence to a preconceived pattern. Since Maddox questions the validity of the work of seven people based upon an

²⁰ *Ibid.*, 164.

²¹ Those who wish to examine Maddox's charges in full detail will eventually have to gather together all the documents and dig through them. Some of the people criticized by Maddox have gone to great lengths to answer his specific accusations, and those answers would be a good place to begin a full and detailed analysis. Maddox's critique of Gar Alperovitz's *Atomic Diplomacy* appeared a few weeks before his book under the title "'Atomic Diplomacy': A Study in Creative Writing," *Journal of American History*, 59 (1973): 925-34. Alperovitz's response is in *ibid.*, 1062-67. See also Maddox's earlier summary of his own book published as "Cold War Revisionism: Abusing History," *Freedom at Issue*, Sept.-Oct. 1972, pp. 3 ff. In addition to the brief and incomplete responses from each of the seven subjects following an enthusiastic review of Maddox's book by Francis Loewenheim in the *New York Times Book Review* of June 17, 1973, three other responses have been privately reproduced and distributed. Although numerous copies are in circulation, the easiest way to obtain one would be to write directly to the three who have written them: Lloyd C. Gardner, Gabriel Kolko, and David Horowitz.

examination of only a tiny segment of their research (he concentrates almost exclusively on the period from the Yalta Conference in February 1945 to the Potsdam Conference in July-August 1945), it would seem fair to apply the same limited examination to his book—for in this sort of situation the accuser must be like Caesar's wife.

That task would be easy except that Maddox has chosen to group a curiously mixed bag. He seems to understand that those revisionists he critiques are "by no means monolithic in their own interpretations," but then muddies the water by ascribing some sort of unity with the assertion that "the New Left revisionists collectively have mounted a formidable attack."²² His potentially useful division of revisionists into "hard" and "soft" categories might have established the philosophical foundation the book so sorely lacks, but his definitions are unsophisticated, and he fails to relate those terms to the people under discussion. The sense of confusion deepens when one reads the opening sentences of the chapter examining D. F. Fleming's study of the cold war:

Pinning labels on historians is a hazardous enterprise at best, but in general the terms "New Left" and "revisionist" are synonymous when applied to interpretations of how the Cold War began. D. F. Fleming is an exception. He wrote his massive, two-volume *The Cold War and Its Origins* as an unreconstructed Wilsonian, not as a critic of the American system as such.²³

Historians should all applaud the definition of labelling as "hazardous"; they can only note with regret that Maddox failed to heed his own advice. Maddox believes Fleming's "interpretations of specific issues have become standard New Left historiography," which seems perilously close to guilt by association since both Maddox and those he attacks agree that Fleming proceeds from ideological premises vastly different from those of Williams, Gardner, Kolko, *et al.* If Fleming is admittedly not New Left, then why is he included in an examination of evidence titled *The New Left and the Origins of the Cold War*?²⁴ The answer seems to be that Fleming is a convenient straw man. The historical profession's opinion of the scholarly reliability of Fleming's study is best indicated by its disappearance from

²² *Ibid.*, 3. The "hard" and "soft" labels are discussed on pp. 4–5. Williams himself seems to recognize the need for careful distinctions. He recently criticized Lloyd Gardner's emphasis on "the extent to which American policy makers were coping with external events rather than moving to impose their will upon reality." W. A. Williams, ed., *From Colony to Empire* (New York, 1972), 6. Maddox's belief that Williams provided the inspiration for "a number of younger scholars—some his own students" (*The New Left*, 13) suggests a quick review of the educational background of the other six authors Maddox discusses. Of the six, only Lloyd Gardner studied with Williams. As Norman Graebner has found: "Wisconsin graduates . . . do not dominate the New Left." Graebner, "The State of Diplomatic History," *The Society for Historians of American Foreign Relations Newsletter*, Mar. 1973, p. 4.

²³ Maddox, *The New Left*, 39.

²⁴ Indeed, Fleming's influence on the others Maddox examines is questionable. Other than a brief historiographical reference Fleming's book is cited in only one footnote and in only one bibliography out of the other six books.

recent bibliographies. Maddox hardly provides a service to the profession by including it in a study of New Left perfidy.

The inclusion of David Horowitz's *The Free World Colossus* in an attack on New Left scholarship is likewise curious. Though Horowitz attached the paraphernalia of scholarly apparatus the historical profession has rarely treated the book as a piece of objective research.²⁵ Neither edition was reviewed in either the *American Historical Review* or the *Journal of American History*. As Maddox correctly points out, Horowitz failed to use many of the printed primary sources, particularly the relevant volumes from *Foreign Relations*. To include an admittedly nonscholarly account in a critique of New Left scholarship is simply invalid.

Maddox's specific accusations begin with an examination of *The Tragedy of American Diplomacy* by William A. Williams. As in all the succeeding chapters Maddox deals with only that tiny portion of the book that posits a strong American interest in Eastern Europe, American violation of the Yalta accords, and a belligerent anti-Soviet policy on the part of Harry Truman. Maddox's complaints center around Williams's use of *ad seriatim* quotations, a method that troubles many historians including some of Williams's own students. There is no question that Williams strings together series of quotations on certain subjects without telling the reader when, where, and why they were said. The failure of Williams and/or the publisher to provide useful citations reduces the scholarly usefulness of the book. Nevertheless Williams's response to Maddox's criticisms is worth quoting: "Historians, like literary critics, sociologists, psychiatrists, or anyone else struggling to make sense out of reality, use seriatim quotations to document, illustrate, and communicate to the reader the substance and texture of the *Weltanschauung* of the protagonists—of the way the actors make sense out of reality."²⁶

While it is easy to attack the stream of consciousness arguments of Williams as a total recreation of the physical or verbal facts, intellectual history—for that is what Williams writes—simply cannot be judged in that way. The probing of human minds requires the accumulation and study of the facts, but the key to true and useful intellectual history—as opposed to mere literary history—is the intuitive ordering of facts and ideas. No one is obliged to accept the interpretation that follows, but disagreement does not justify name-calling. Nor is Maddox accurate even in his footnote checking. He states that "Williams argued that the United States itself bore the primary responsibility" for bringing about the cold war. In fact,

²⁵ Of the others treated in Maddox's book none used *The Free World Colossus* as a source.

²⁶ William A. Williams, *New York Times Book Review*, June 17, 1973, p. 7. Williams's use of *ad seriatim* quotations can and has been effectively challenged, but only when the challenge is based on a full, scholarly appreciation of Williams's avowed methodology—not on the basis of shadowy allegations about motives. See J. A. Thompson, "William Appleman Williams and the 'American Empire,'" *Journal of American Studies*, 7 (1973): 91–104, for a balanced assessment of Williams's methods and conclusions.

Williams specifically denies that notion by pointing out that to say the United States “crystallized the cold war . . . is not to say that the United States started or caused the cold war.” Rather the cause was Hitler’s destruction of the existing structure in Eastern Europe. What Williams specifically rejects is not the idea of blaming the Soviet Union for contributing to cold war tensions, but the claim that Russia was *solely* to blame.²⁷ Given the state of conventional historical wisdom in 1959, when Williams’s essay was published, it is not surprising that he paid less attention to Soviet than American contributions to the cold war.

The most serious flaw in Maddox’s reading of Williams’s argument, however, is his failure to comprehend how Williams uses the term “Open Door.” He notes that Williams claims the policy had become “internalized,” hence there was little need to discuss or defend it. Maddox claims Williams then contradicts himself by using “copious quotations” to show how Henry Stimson and W. Averell Harriman had to tutor Truman and his secretary of state, James Byrnes, about the Open Door in East Asia; for if the policy were so completely “internalized,” Maddox argues, why would there be any need for further persuasion? That argument simply ignores the enormous difference between Williams’s broad use of the phrase “Open Door” to describe the basic thrust behind American foreign policy and the discussion of tactics as well as the education of an inexperienced president concerning the traditionally held Open Door in China. The use of one term to describe two different levels of thought and action is both annoying and confusing, but that hardly warrants accusing Williams of “pervasive misusages of source materials.”²⁸

Equally interesting is that, while Maddox claims not to be concerned with differences in interpretation, it appears that just such differences lie at the heart of his criticism of Williams. Even when seemingly merely checking footnotes Maddox essentially deals with interpretations. In dismissing a summary of the foreign policy dispute between Henry Wallace and President Truman as “solemn nonsense,” Maddox disagrees with Williams’s interpretation of the basic nature of Truman’s policy toward the Soviet Union. Maddox correctly points out that Wallace did not specifically refer to a loan to Russia in his letter of March 14, 1946, but the secretary of commerce did characterize the overall tone of American-Russian relations, and that general characterization is the issue with which Williams was concerned.²⁹

Maddox begins his discussion of Gar Alperovitz’s *Atomic Diplomacy* by stating that the “book has become a staple of ‘New Left’ historiography.” That is a curious position since Clemens, Gardner, Kolko, and Williams—either specifically or by inference—have rejected Alperovitz’s basic theme

²⁷ Maddox, *The New Left*, 14; Williams, *Tragedy of American Diplomacy*, 206–07.

²⁸ Maddox, *The New Left*, 16–19, 10.

²⁹ *Ibid.*, 34–36.

that Harry Truman radically altered the cooperative policies of Franklin Roosevelt toward Russia.³⁰ Since, as the title suggests, Alperovitz's entire thesis stands on the belief that Truman reversed Roosevelt's policies by practicing atomic diplomacy, Alperovitz seems to have "become a staple of 'New Left' historiography" only in the negative.

To return again, however, to the specifics of footnoting, Maddox claims that "one of the most common flaws in the book is Alperovitz's practice of citing statements in support of his arguments which, in context, refer to other subjects altogether." Truman's statement that the Russians "could go to hell," associated by Alperovitz with the president's views on the question of the reorganization of the Polish government, refers, according to Maddox, to the question of boycotting the San Francisco conference. Since Maddox follows by agreeing that Truman understood the Russians might boycott the conference if they did not get their way on the Polish question, Maddox seemingly contradicts himself.³¹

In spite of repeated protestations to the contrary,³² Maddox deals primarily with interpretations—not falsification of the evidence. As Alperovitz showed in his rejoinder to Maddox's critique their differences related to the use of ellipses and the relationship between specific statements and broad policies. For example, Maddox claims that Alperovitz distorted history by applying a statement of Averell Harriman to Lend-Lease when it actually referred to postwar credits to Russia. That is simply not what Alperovitz did. Granted, Harriman was referring to postwar credits and not Lend-Lease, but as anyone who has studied economic diplomacy during this period knows, the extension of Lend-Lease and the question of postwar credits and loans were essentially one in the same.³³ Maddox may well, on the basis of the evidence, disagree with the theory that the United States used economic aid as a club and thus helped precipitate the cold war, but he must also permit others to accept that view. It is worth noting that Professor George Herring, who is cited by Maddox as having rebutted "the notion that Lend-Lease curtailment was designed to coerce Russia," strongly agrees that Harriman developed a "sophisticated strategy" that, while oversimplified by the left revisionists, nonetheless included economic coercion as a key element. Herring, whose book is an admirable example of original research that arrives at an eclectic combination of orthodox and revisionist interpretations, demonstrates that the actual abrupt halting of Lend-Lease to Russia was not part of any overall plan for coercion, yet he also argues

³⁰ Kolko, *The Politics of War*, 421–22, 538–43; Williams, *Tragedy of American Diplomacy*, 239, 244; Gardner, *Architects of Illusion*, 305–06; Clemens, *Yalta*, 279; Maddox, *The New Left*, 63.

³¹ Maddox, *The New Left*, 65.

³² Statements by Robert J. Maddox at the Columbia University American Studies Seminar, October 18, 1973.

³³ Letter to the editor from Gar Alperovitz, *Journal of American History*, 59 (1973): 1062–67; Maddox, *The New Left*, 66–67; Maddox, "Atomic Diplomacy," *Journal of American History*, 59 (1973): 927.

that just such a general plan did exist even though it was only sporadically carried out.³⁴

As with Alperovitz's book, Gabriel Kolko's *The Politics of War* presents a highly controversial and rigidly applied thesis about American foreign policy and diplomacy during World War II. And, as with his examination of Alperovitz's book, Maddox's critique is so narrowly conceived that it cannot come to grips with the broad context within which most of Kolko's arguments are made. Though Maddox claims to be checking footnotes his case actually rests upon his own interpretation of admittedly ambiguous documents. In a treatment of "Kolko's most striking misuse of evidence" Maddox rests his entire argument on what was meant by the Yalta agreement to give the Poles "substantial accessions" of land in the west. Kolko argues that Truman's opposition to the Soviet proposal of the Oder-Neisse boundary reversed "clear commitments" made at Yalta. Maddox, calling Kolko's account "bizarre," asserts that no such clearcut agreements were made at the Crimea Conference. In fact, if one consults the basic documents—the Yalta Papers—the entire situation becomes most ambiguous. Churchill's only stated objection to the Oder-Neisse boundary was the presence of a large German population that would have to be transferred. When Stalin answered that most of those Germans had fled before the Red Army, Churchill remarked that that "simplified the problem." Although Churchill later repeated that objection, no other substantive arguments were made at Yalta against the Soviet proposal. Kolko assumes that Stalin interpreted all that as tacit approval of his suggestion; Maddox assumes that the Anglo-American refusal to consent formally to the Oder-Neisse line made it clear to Stalin that "substantial concessions" meant only up to the Oder River.³⁵ One could easily quarrel with Kolko's claim that Roosevelt and Churchill had made "*clear commitments*" regarding the Oder-Neisse boundary, but Maddox eschews any such argument over interpretations. A narrow examination of footnotes in the absence of a full understanding of the entire context of a book is an inadequate critique. To make a proper determination of such arguments as Truman's alleged violation of the Yalta agreements requires the exercise of what George Dangerfield has recently called "historical imagination"—the ordering of evidence based upon the broad context as well as the specifics of the event.³⁶ Based on this

³⁴ Maddox, *The New Left*, 70–71n.; Herring, *Aid to Russia*, 208–211, 290–91.

³⁵ Maddox, *The New Left*, 117–18. Kolko, *The Politics of War*, 576–78. Kolko points out—and the documents verify—that the United States opposed any cession of land to Poland west of the Oder River, but would not insist on that point should both Russia and Britain agree on the transfer. Kolko never claims that there was any specific, unequivocal agreement made at the Yalta Conference about the Oder-Neisse line; see *ibid.*, 356–57, 403. The documents are in U.S. Department of State, *Foreign Relations of the United States* (hereafter *FRUS*), *Conferences at Malta and Yalta, 1945* (Washington, 1955), 716–17, 725–26, 898–99, 911–13, 917–18. Though my own reading of those documents differs from that of Kolko, it does not support a finding of conscious distortion in *The Politics of War*. See also, *FRUS, Conference of Berlin (Potsdam), 1945*, 1 (Washington, 1960): 750–51.

³⁶ Dangerfield in the *New York Times Book Review*, Oct. 28, 1973, p. 2.

limited type of investigation Maddox—with an ambiguity that would have made Franklin Roosevelt proud—casts doubts on Kolko's veracity:

In his acknowledgments to those who had aided him in preparing his manuscript, Kolko called special attention to a colleague's "critical commitment to seeking truth. . . ." "Whole sections" of *The Politics of War*, according to its author, were written in response to that commitment. The response, one must conclude, was singularly inappropriate.³⁷

Maddox's treatment of Diane S. Clemens's *Yalta* closes with a similarly worded inference of conscious dishonesty. After mentioning a "multitude" of misuses of evidence, he claims that the frequent occurrence of such errors, "which invariably lend the appearance of substance to her themes, renders implausible the explanation of mere carelessness." Though he gallantly mitigates that attack by suggesting "'overzealousness' is the most charitable word," there is no doubt as to Maddox's meaning.³⁸

As I have stated elsewhere I disagree with Clemens's thesis,³⁹ but that is a far cry from lightly veiled aspersions about her academic ethics. As for the substance of Maddox's critique once again he fails to distinguish between differing interpretations and dishonesty. Again, a single example. Clemens and Maddox each look at a statement by Harry Truman and interpret it differently. Maddox's approach is to look narrowly at the exact quotation, which concerns the Yalta agreement to reorganize the Lublin government in Poland. Clemens writes that Truman demanded a "new" government, whereas the Yalta agreements called for only a "reorganized" government. Maddox quotes both the Yalta agreement and Truman's note to Stalin and compares just the two documents. His conclusion is that Truman merely paraphrased the Yalta agreement on Poland and was only asking that Stalin live up to his promises. Clemens finds a different emphasis in Truman's note. She argues that the Yalta agreement called for the reorganization of the Lublin government and later referred to that broadened structure as a "new" government, whereas the Truman note called for the reorganization of the provisional Lublin government "in order to establish a new government."⁴⁰ One could argue endlessly as to whether or not Truman's word order constituted a proper paraphrase or a substantive change, but the only way to draw any valid conclusion is to exercise that indispensable "historical imagination." The diplomatic historian must always view the documents within their overall context, and mere semantic arguments are not persuasive. Maddox may not agree with Clemens's perspective, but he claims that he is not concerned with perspective and interpretations—only footnotes.

³⁷ Maddox, *The New Left*, 121.

³⁸ *Ibid.*, 137.

³⁹ Review in *International Journal*, 26 (1971): 812–15.

⁴⁰ *FRUS, Yalta*, 980; *FRUS*, 1945, 5 (Washington, 1967): 258; Maddox, *The New Left*, 125–26; Clemens, *Yalta*, 269.

Maddox saves Lloyd C. Gardner's *Architects of Illusion*, the New Left's "most sophisticated and convincing account" of the origins of the cold war, until last, but the conclusions are the same. "Subtler and more persuasive than previous revisionist works, . . . it is history by irrelevant evidence, misused quotations, jumbled figures, and distortions of documentary materials. Gardner . . . squandered his gifts."⁴¹ Gardner certainly must have been quite sophisticated, for the first six pages of Maddox's twenty-page essay deal with what he claims he is not dealing with—interpretations. The conclusion of that six-page discussion is that "the lack of hard evidence caused Gardner's broader thesis to suffer from malnutrition."⁴² In spite of his disclaimers Maddox's analysis of Gardner's positive evidence is likewise an exercise in differing interpretations. Again a single example will suffice. Maddox chides Gardner for incorrect placement of the word "only" in a statement about the decisions on Poland made at Yalta. Since the correction is immediately preceded by the statement that Gardner has "made significant contributions" to "revisionist legendry" and persistently misrepresented what happened at the Yalta talks it would appear Maddox believes this to be an example of such a "contribution."⁴³ Gardner wrote: "While F.D.R. did insist at this plenary session that there be free elections as soon as possible in Poland, the final protocol of the Yalta Conference stated *only* that the new Polish provisional government should be a 're-organization' of the Lublin government."⁴⁴ As Maddox points out, "That is not what the protocol 'only' stated," since it also contained a pledge that the Polish provisional government would hold free elections.⁴⁵ Although Maddox does not comment further the reader is obviously led to believe that Gardner ignored the free election pledge because it did not fit his thesis. As with almost all of Maddox's charges against *Architects of Illusion* he ignores the context and point of the argument Gardner was making. In this case Gardner seems to be emphasizing that Roosevelt had assented to only a reorganization of the Lublin government rather than a completely new start. Therefore the American meaning of a "free election" had already been compromised. One can gently criticize Gardner for a poorly constructed sentence, but not for any sort of purposeful distortion.⁴⁶

Maddox's concluding chapter is largely taken up with a condemnation of orthodox reviewers for their failure to check the evidence, as he has, and

⁴¹ Maddox, *The New Left*, 139, 158.

⁴² *Ibid.*, 144.

⁴³ Maddox, *The New Left*, 149.

⁴⁴ Gardner, *Architects of Illusion*, 52 (italics added).

⁴⁵ *FRUS*, Yalta, 980.

⁴⁶ If Gardner did create a piece of "revisionist legendry" by seeming to claim there was no promise of free elections, it was a short-lived legend even in Gardner's own writings. In a recently published text written before Maddox's book appeared, Gardner stated: "At the completion of the reorganization process the new Polish government was to plan for free and democratic elections—a perfect solution in theory, but one unlikely ever to be realized." If anything, that statement implies a criticism of Soviet policies. Lloyd C. Gardner, Walter LaFeber, Thomas McCormick, *Creation of the American Empire: U.S. Diplomatic History* (Chicago, 1973), 440.

for recommending that his own work not be published. He suggests two explanations of why orthodox historians have, in effect, endorsed revisionist works by failing to review them rigorously. The first, that reviewers were not qualified academically to examine a certain book, is obviously, though one hopes infrequently, valid. "The second possibility—far more intriguing—is that reviewers who were perfectly aware of the procedures employed, nevertheless concluded that it was unnecessary to share this information with their readers."⁴⁷ Since Maddox's book is based on the premise that "the procedures employed" were "*without exception . . . pervasive misusages of the source materials*," he has accused what is probably a majority of his peers of consciously aiding and abetting in the falsification of history. In short, they are frauds!⁴⁸

As if such insinuations were not enough Maddox also broadly hints at mundane motives among revisionist historians. By writing that "it is certain that these 'bold' and 'provocative' interpretations have earned fame and academic advancement for their authors"⁴⁹ he obviously intends to plant a seed of doubt in the reader's mind. We all, including Professor Maddox, are gratified by any "fame and academic advancement" that our efforts bring, but to infer that the successful have prostituted their intellectual standards in order to achieve that success is tasteless and unethical. The left revisionists can and have been effectively and searchingly criticized,⁵⁰ but the Maddox book is not an example of how to do it. Left revisionism poses real questions of methodology—in the philosophical as well as the technical sense—for all of American history, but Maddox ignores them. Though he claims to demonstrate the poverty of left revisionist scholarship, he actually only demonstrates the poverty of his own evidence and logic.

At present all too many historians of the cold war seem inclined to follow that same self-destructive process they and their predecessors took after World War II when revisionism and its opponents battled over the question of Franklin D. Roosevelt's alleged dishonesty, deceit, and dissembling.⁵¹ This time the dispute is over the origin of the cold war instead of American

⁴⁷ Maddox, *The New Left*, 161.

⁴⁸ *Ibid.*, 10. Maddox offers a possible motive for such actions in "Revisionism and the Liberal Historians," *Freedom at Issue*, May-June, 1973, pp. 19-21. He theorizes that liberal historians have a sense of guilt arising out of the Vietnam War and their support of policies—such as Wilson's Fourteen Points—which contributed, according to the revisionists, to that war.

To justify his accusations about liberal historians, Maddox relates some of his own experiences in trying to publish his work. *The New Left*, 161-63. His argument is, according to the then editor of the *AHA Newsletter*, John J. Rumbarger, based solely on Maddox's attempt to publish a portion of his book in the *Newsletter*. Rumbarger claims that Maddox completely distorted the thrust of the various readers' comments. Rumbarger's version of the story, "Robert J. Maddox and the Use of Evidence," may be obtained directly from Rumbarger.

⁴⁹ Maddox, *The New Left*, 159.

⁵⁰ For example see Robert W. Tucker, *The Radical Left and American Foreign Policy* (Baltimore, 1971), and the previously cited article by J. A. Thompson, "William Appleman Williams," *passim*.

⁵¹ The places to begin an examination of this literature are Justus D. Doenecke, *The Literature of Isolationism: A Guide to Non-Interventionist Scholarship, 1930-1972* (Colorado Springs, 1972), and Warren I. Cohen, *The American Revisionists* (Chicago, 1967).

entry into wars against Japan and Germany, but the temper of the debate has taken on the same vituperativeness. The evidence has begun to mount steadily. At a recent historical meeting a revisionist was accused of consciously misleading his students. Another revisionist has recently circulated a private (not for attribution) memorandum that unquestionably infers the existence of a conspiracy to discredit his and similar critiques of American foreign policy. Professor Oscar Handlin asserts that revisionist arguments are "inherently absurd" and in print only because they are "the most salable commodity."⁵² During a recent Columbia University Seminar, which featured Robert J. Maddox as the speaker, one member of the audience angrily called Maddox "a crude polemicist and hack." The *New York Times Book Review* editor apparently saw the possibilities of such bickering when he sought out a strongly opinionated reviewer who elaborated on and even exaggerated Maddox's arguments against the New Left, followed by brief retorts from each of the seven authors Maddox criticized. Most of those responses only heightened the unscholarly level of rhetoric and name-calling.⁵³

Deep, emotional, personal involvement is, unfortunately, a characteristic among historians of contemporary history—particularly when they deal with the compelling and almost pre-emptive issue of war. The results of such intense subjectivity have often been tragic. Preoccupation with the mendacity of Franklin D. Roosevelt virtually destroyed the creditability of such seminal thinkers as Charles A. Beard and Harry Elmer Barnes and completely discredited an indefatigable researcher like Charles C. Tansill. (Beard, shortly before his death, stated that he wanted to write one more book about that "son of a bitch," Roosevelt.) In spite of occasional insights, numerous hard and valid questions, and a potentially viable thesis, these and other historians vitiated their own arguments by their intemperance and nastiness.

The philosophers and essayists who wrote in the genteel tradition were quite right when they claimed that style is often as important as substance. It is not hypocritical to disagree with someone in a gentle, courteous manner; rather that is how human society must function if it is ever to achieve peaceful relationships. Walk into a barroom with a chip on your shoulder and surely someone will knock it off. So it is with historians: intellectual belligerence merely begets more of the same. Even candor and bluntness can be couched in honest but mannerly terms. Granted, it is far easier and much more fun to be vitriolic and sarcastic, but such writing hardly qualifies as a dispassionate search for truth; rather it merely polarizes the argument.

Unfortunately the effects of *ad hominem* and belligerent historical disputes are far wider than just the immediate participants; our students and the

⁵² Oscar Handlin, "Revisionist History: A Base for Neo-Isolationism," *Freedom at Issue*, Sept.-Oct. 1972, p. 2.

⁵³ Francis Loewenheim, review of *The New Left and the Origins of the Cold War* by Robert J. Maddox, in the *New York Times Book Review*, June 17, 1973.

general public are likewise affected. Generations of post-World War II graduate students were brought up believing that Barnes, Tansill, and Beard had prostituted themselves and their professional standards. As a result few students in the fifties or sixties read their work except to mock or scorn it in an historiographical paper for their methods course. Barnes's paranoid claims of a blackout conspiracy became a self-fulfilling prophecy as the consensus of the historical profession condemned him and others who indulged in intemperate and personal attacks on Roosevelt. Yet the quiet, positive, and persistent critiques by the late William Neumann—writings that raised all the important questions but never transgressed the bounds of taste and manners—were consistently cited as an example of moderate (if incorrect) revisionist scholarship.

We can hardly expect our students to understand or believe in the importance of civility and respect for the opinions of others if their teachers and intellectual leaders ignore it. Historiographical warfare is no substitute for scholarship; dark hints of conspiracy should not replace the awareness that our opinion might be wrong; name-calling and sarcasm must never be confused with careful criticism.

Reviews of Books

GENERAL

L. G. HELLER. *Communicational Analysis and Methodology for Historians*. New York: New York University Press. 1972. Pp. xii, 179. \$12.00.

Heller wishes to set forth the methods and objectives of linguistic and graphemic reconstruction. His book lacks an index, uses, in part, idiosyncratic approaches and a private terminology, and sometimes stops short of the essential point. It does touch on most of the topics that one would want to see included.

Reconstruction may establish a point in time at which an ancestral speech community split apart, or at which languages had contact with one another; techniques range from "simple dating procedures" to the sophisticated, if uncertain, inferences of glottochronology (based on assumptions about the persistence of vocabulary in time). In addition to such "Chronological Reconstruction" (chapter 2) there is "Geographical Reconstruction" (chapter 3), aimed at fixing the ancestral habitat and at tracing migration routes and the like. There follows (chapter 4) a discussion of the nature of language and change. How effective this is remains to be seen; the statement about reconstruction resting on the "observation that linguistic changes admit of no exception whatever" will cause confusion or pain or both—not to be relieved by the few lines of explanation that follow. The fifth chapter returns to the retrieval of meanings, this time of sociocultural ones like kinship vocabularies. Chapter 6 contains reflections on the possibility of knowing the personality and the personal status of the writer of a text. There is a chapter on decipherment (chapter 7) and a disquisition on external (nonlinguistic) causes of linguistic

change, illustrated not as one might expect with the usual references to substrata, area typologies, etc., but with a rather imaginative sketch of the background of the formulas of address (thou, ye) in some European languages. In chapter 9 the reader is promised a glimpse of the frontiers of linguistics and told about the rank-frequency law that is rightly described as a statistical concept of wide application in a number of fields but presented in a completely non-Mandelbrot version.

With allusion and example Heller succeeds in stimulating interest, but hardly in giving his reader a sense of the characteristic formal concreteness of linguistic procedures and of their power and limitations.

HENRY M. HOENIGSWALD
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KAJ BIRKET-SMITH. *Eskimos*. New York: Crown Publishers. 1971. Pp. 277. \$17.95.

Danish explorer-scholars were among the first to begin the scientific study of Eskimo culture nearly one hundred years ago, and they remain today among the most informed sources of information on Eskimo anthropology and archeology. Birket-Smith is of the best of the Danish school that has left the distinctive imprint on work in this field. A lifetime of research into nearly every aspect of Eskimo culture qualifies him pre-eminently for the task he has here set forth.

Eskimos is a unique scholarly study of the most fascinating and widespread of the world's ethnographic peoples. Stretching from southwest Alaska to southern Labrador and Greenland they encompass a culture area of some four thousand miles linear distance—surely the

greatest area occupied by any ethnographic group sharing a basic language and culture type. Yet within this unity there is important variation that Birket-Smith manages to incorporate within his catholic grasp of detail and compelling personal and narrative style. We learn of Eskimo subsistence adaptations, technology, religion, language, social organization, and mythology with a marvelous blend of historicism and personal experience; of Eskimo contacts with Siberia and medieval Europe, of the Norse colonization of Greenland and subsequent effects on its local Eskimo inhabitants, and of the important question of Eskimo origins.

This much has been told before, as the present Crown edition is basically a republication of Birket-Smith's 1959 *Eskimoerne*, which has also been available in English for some time. The major additions to this edition are the large format, the striking color and black-and-white illustrations, and the incorporation of part of a formerly published essay on the present status of Eskimo society in the modern world by the recently deceased Canadian anthropologist, Diamond Jenness. Jenness's eloquent plea for rationed approaches to modern problems in the north is one of the finest pieces of literature a concerned humanist has ever produced in the field of anthropology, and its inclusion together with new illustrative material in this edition makes the work the most communicative statement available on Eskimo culture, history, and society in a day when Arctic development is no longer a remote frontier. Unfortunately Jenness's compassion is heightened by Birket-Smith's now outmoded use of such concepts as "infantile" and "primitive," regarding Eskimo cultural and physical anthropology and psychology (pp. 46, 58), and his reliance on antiquated diffusion theory, "cultural levels" (p. 213), and evolutionary determinism betrays a conjectural history approach to culture that has marked the Danish school of Eskimo ethnography. In addition the continued belief in his now disproven theory of Eschato-Eskimo (interior) origins takes little account of present evidence, furthering the immensity of the generation gap in Eskimo studies and pointing glaringly toward the need for a new synthesis.

WILLIAM FITZHUGH
Smithsonian Institution

VERN L. BULLOUGH, with the assistance of BONNIE BULLOUGH. *The Subordinate Sex: A History of Attitudes toward Women*. Urbana: University of Illinois Press. 1973. Pp. viii, 375. \$10.95.

Anyone attempting to write a one-volume *History of Attitudes toward Women* shows, as the author concedes, "a kind of foolhardiness which is usually condemned in scholarly circles." Vern Bullough, a professor of history at California State University at Northridge, attempts to deal with this difficulty by citing the lack of research in this field and by including, presumably for the feminine point of view, a closing chapter by his wife, Bonnie Bullough, a nurse and sociologist. Predictably, this purely formalistic approach at remedying a basic conceptual weakness fails, and we are left with a textbook based on frequently inadequate secondary sources, superficially treated and marred by present-minded slanting. What is wrong is not the author's sex, but his simplistic concept of women's history as nothing more than a compilation of "compensatory" facts and an endless recitation of suffering, victimization, and oppression. Again, the title, *The Subordinate Sex*, expresses the author's concepts and their limitation quite adequately. To view women as "the subordinate sex" is to disregard their continuous contribution to the building of human culture, to ignore their unique and separate historical experience, and to fail to come to grips with the essential conceptual implications of including women in history, namely the challenging of male-defined concepts of what history is. Women's history is not the history of male attitudes toward women, nor is it the history of women as victims any more than it is the history of women as wives and daughters of the true "makers and shapers" of history. The wholesale omission of women from the work of historians cannot be rectified by compensatory listings of "achievements" and by a catalog of discriminatory practices. It cannot be adequately dealt with by including the concept of "sex and sex role indoctrination" in historical inquiry, although that is a worthwhile and helpful inquiry. Certainly, the injection of the terminology of the women's liberation movement can only serve to give the gloss of contemporary topicality, possibly in the hope of enhancing sales, but it cannot be taken seriously as scholarship.

Still, this book has its usefulness. As a survey

of the way in which men have regarded women it summarizes the social, legal, and economic status of women in various periods of history, ranging from antiquity to the present and including the major cultures, with the notable exception of Africa, but including Islam. As a scholarly contribution to the field it has nothing to recommend it. Given the absence of primary sources, the lack of a bibliography, and the superficial nature of the "Guide to Further Reading," it is very doubtful that it "might serve as a guideline for monographic studies." Those desiring guidelines that lay no claim to historical scholarship but abound in stimulating and original concepts valuable to the scholar might do better to consult the work of Simone De Beauvoir, Elizabeth Janeway, Mary Beard, and Viola Klein.

GERDA LERNER
Sarah Lawrence College

OWSEI TEMKIN. *Galenism: Rise and Decline of a Medical Philosophy*. (Cornell Publications in the History of Science.) Ithaca: Cornell University Press. 1973. Pp. xvii, 240. \$15.00.

For well over a millennium medicine in Western and Central Europe (around the Mediterranean basin) and in the Middle East was pervasively influenced, and to a greater or lesser degree dominated, by the system called "Galenism." In 1865 the French medical historian, Charles Daremberg, noted that as late as the middle of the eighteenth century this system persisted almost in its entirety despite discoveries in anatomy, physiology, and pathology that invalidated its fundamental premises. Nonetheless this historical phenomenon has not attracted the attention and investigation it merits. The physician Galen (ca. A.D. 130–ca. A.D. 200) has been and is being studied by medical historians, classicists, and Arabic scholars, yet the system that Galen did not create, but that was developed from his teaching and his writings, still awaits equivalent attention. All the more welcome, therefore, is this study of Galenism by Professor Temkin. Originally presented in 1970 at Cornell University as the Messenger Lectures the published text has been considerably expanded and revised.

Temkin considers Galenism "as an intellectual phenomenon, as a philosophy in the sense of principles, beliefs, and facts, more or

less cogently connected to form a set and ascribed to Galen." The rise and decline of this medical philosophy, the vicissitudes of Galenism from its appearance in the later Roman Empire to its disappearance in the eighteenth century, are delineated sympathetically, clearly, and, as one expects of Temkin, with an obvious but unobtrusive command of a wide variety of primary and secondary sources.

Galen marks both an end and a beginning in the history of Greco-Roman medicine. He was very well acquainted with the writings of his predecessors as well as with those of his contemporaries and examined critically contemporary and past currents of thought and practice. In this sense Galen's works are a huge repository of ancient medical literature as well as of natural philosophy and were thus able to provide the basic substance for Galenism. Moreover Galen's reputation as a physician and natural philosopher grew after his death, and by A.D. 350 his place as the leading authority had been established. Temkin analyzes carefully and in detail the intellectual strands out of which the fabric of Galenism was woven and in passing refers to the period of civil strife, unrest, and insecurity, occurring between the death of Commodus and the conversion of Constantine, as having provided a receptive environment for Galenism. One may suggest in this connection that the emergence of Galenism paralleled and probably reflected the standardization and systematization of civic and cultural life that took place during this period. As H. P. L'Orange demonstrated the emergence of a trend toward simplified, static forms in art and architecture in the late Roman Empire accompanied the establishment of a new political structure. A similar process in medicine and natural philosophy, a movement away from the empirical and the relative to the dogmatic and the authoritarian, a movement toward uncomplicated absolutes in systematic form, produced Galenism. But as Temkin points out there is a scarcity of material for a century and a half following Galen's death so that the early development of Galenism remains obscure and requires further investigation.

Temkin traces the spread of Galenism as it traveled initially through the Greek centers of learning, Alexandria and Constantinople, rather than by way of Syria and the Muslim countries. Until the eleventh century there was

little Galenism in the Latin West, but thereafter Arabic influence made itself felt in Europe and the knowledge of Aristotelian natural science and metaphysics spread. But Galenism did not remain unchallenged. Rhazes, Ibn an-Nafis, Averroes, and Maimonides criticized aspects of Galenism, yet doubts and deviations from accepted doctrine were not fatal as long as physicians and the lay public found their way within the structure of the system. The decline of Galenism was not a cataclysmic event; it was a slow process due not to a single cause, but the interaction of a number of conditions. It survived into the eighteenth century because the new science could not provide a guide to medical practice while traditional methods did.

The rise, decline, and fall of Galenism as presented in this interesting and well-written book should be of interest to a large audience: medical historians; historians of ideas and of culture; classicists and medievalists; and historians of science, philosophy, and religion. Naturally those already familiar with some aspect of the subject will judge the author's achievement from their viewpoints. For the thoughtful reader, whatever his or her background, this original contribution by an outstanding scholar offers much of interest within a small compass.

GEORGE ROSEN
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R. A. G. CARSON, editor. *Mints, Dies and Currency: Essays Dedicated to the Memory of Albert Baldwin*. London: Methuen and Company; distrib. by Barnes and Noble, New York. 1971. Pp. xv, 336, 23 plates. \$33.50.

This *Festschrift* volume is dedicated to an unusually competent coin dealer who worked closely with scholars and museums. The volume is handsomely printed on heavy paper and the plates are excellent. The book has all the usual virtues and defects of *Festschriften*: the contributions are uneven in length and in value.

There are two appreciations of Baldwin. The scholarly contributions are arranged chronologically and range from the period of the Roman Republic to the sixteenth century,

with heavy emphasis in the later articles on Great Britain.

The material on ancient coinage is mostly excellent. Charles A. Hersh has published some additions to the E. A. Sydenham catalog, *The Coinage of the Roman Republic*, in continuation of his earlier contributions. Some of the coins illustrated are not to be seen elsewhere. It is rather surprising that not he but Michael Crawford of Cambridge has produced a revision of the Sydenham corpus, and one hopes that Crawford had this information available to him. Curiously Crawford's forthcoming work is not mentioned by Hersh.

P. V. Hill's "The Dating and Arrangement of Hadrian's 'Cos III' Coins" bears upon the dating of the travels of that emperor; there are implications for other historical assessments of Hadrian's personality and policies. The article must be consulted by any future biographer. Carson's own chapter, "The Sequence-marks on the Coinage of Carausius and Allectus," also a continuation of earlier work, is a model of proper procedure to be used in examining a corpus of coins that include anomalous specimens.

As one would expect, some of the more important contributions deal with the coinage of Great Britain. "Variations in Currency in Late Anglo-Saxon England" by C. S. S. Lyon will be interesting to all numismatists because of his discussion of some of Sture Bolin's theses on coin weights and overvaluation. Interesting questions are raised that are yet to be answered on such matters as mint charges and exchange of obsolete coins.

The longest contribution is Ian Stewart's "Scottish Mints," a kind of supplement to the author's *The Scottish Coinage*. The earlier sections on the mints contain some historically interesting material, as for example the observation that the main mints tended to be shifted to sites strategically important in the contemporary historical context. For the general numismatist a most important section is that dealing with die-links. Some of these are die-links between mints. The possibilities in these relationships are detailed. A quite valuable and judicious section discusses the technical problems involved in identifying die-links, which is not as simple a matter as it may seem. Dies wear down or are dressed down, the

striking angle may vary, and the depth of the relief may cause varying appearance in the specimens.

Several of the other contributions are well done, though some are slight. All research libraries should acquire the book.

HENRY C. BOREN

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ARNE BERG *et al.* *Architecture in Wood: A History of Wood Building and Its Techniques in Europe and North America*. Edited by HANS JÜRGEN HANSEN. Translated by JANET SELIGMAN. (Studio Book.) New York: Viking Press. 1971. Pp. 288. \$40.00.

This lavishly illustrated, handsomely produced, and expensive volume has the appearance of a coffee-table book. In reality it is both more and less. The editor, a well-known Continental scholar, collected a series of essays on building in wood from authorities in Europe and North America in their respective areas, and, with the exception of the contributions of J. T. Smith on the British Isles and Nancy Halvorson Schless on the United States, all are translated from the German. The original version was published in Hamburg. In terms of scholarly merit the best is unquestionably the section on "Wood Building in Northern Europe," by M. N. A. L. Berg, architect of the Norsk Folke-museum in Bygdoy and the Toini-Inkeri Kaukonen, superintendent of the Suomen Kansallismuseo in Helsinki, and the editor. It would be hard to find a better or more succinct discussion of the famous stave churches of Norway, and the illustrations of the huge wooden churches of Finland are particularly welcome because they are so little known. The English material is also good but less well organized, and the sections on Central Europe and Russia, while provocative, are downright confused. The section by Nancy Halvorson Schless suffers from an overemphasis on the colonial work of the Eastern seaboard. While there is a nod toward Frank Lloyd Wright's extraordinarily creative use of wood in his Usonian houses, something should certainly have been said about the pioneering work of Ellsworth Storey in the Seattle area and the more recent, and extremely influential, homes and churches by Pietro Bel-luschi in Oregon. Wood has always been the

great American building material, and this point deserves to be emphasized.

With regard to the broad historical reach of the book, the most surprising omission is unquestionably the lack of any discussion of the theories of Professor Walter Horn, who holds that the essential structure of the Gothic cathedral was derived from the bay divisions of the great medieval barns. While Horn's evidence for this position is by no means universally accepted, his contentions should at least be mentioned. Almost equally surprising is the omission of any discussion of the major surviving medieval constructions in wood of a monumental nature, notably Westminster Hall and the crossing tower at Ely Cathedral. Clearly, in England during the Middle Ages the master carpenter was generally as important as the master mason, and since we possess a few excellent examples of his skill, these should be illustrated and discussed.

Most aggravating of all is the juxtaposition of photographs and drawings with regard to the text. Granted that the integration of text and visual material is always a major problem in an art historical work, the arrangement which we have here is so discordant as to be infuriating. Thus in chapter 2 the photographs skip, with marvelous illogic, from: (a) a barrel vault in the Church at Oosthuizen, North Holland, 1520 to (b) roof timbers at Santa Croce in Florence, fourteenth century, to (c) the interior of the Guildhall of the Lord Leicester Hospital, Warwick ca. 1390 to (d) the interior of the Breton church of Ste. Catherine, fifteenth century. Each photograph is excellent and would be exceedingly meaningful if it were tied in with the appropriate text (for example, the church at Honfleur with chapter 5, "Wood Building in France"). This kind of haphazard selection reduces the book to a kind of hideous jumble and often negates its usefulness.

In sum, this volume is a useful source for anyone interested in architecture in wood, which is indeed an important and too often neglected topic, but it is so badly put together that its utility is vitiated. It remains to be noted that each chapter has a good bibliography and that it is well indexed.

LEONARD K. EATON

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RALPH DAVIS. *The Rise of the Atlantic Economies*. (World Economic History.) Ithaca: Cornell University Press. 1973. Pp. xiv, 352. Cloth \$12.75, paper \$5.95.

This work will be welcomed as the best text we now have on preindustrial economic history. Its brilliant organization of complex materials, its unabashed eclectic approach, and its highly readable prose make it just the thing for courses in both Old Régime and economic history. It also can be used for adjunct reading in American and Latin American colonial history. And it is perfect for graduate students boning up for an exam in early modern history; they will turn to this from volume 4 of the *Cambridge Economic History of Europe* (and from volume 5, if it ever appears) with a sigh of relief. The series World Economic History, edited by Charles Wilson, of which the Davis work is the first volume to appear, is off to a good start.

"The rise of the Atlantic economies" is here presented as a semitopical, semichronological description of the main changes in the economic affairs of England, the Netherlands, Spain, Portugal, and France (and their colonies in North and South America), from the mid-fifteenth to the mid-eighteenth century. Chapters on the general economic history of these countries and empires are interlarded with other chapters or specialized sections on population, income, commerce, agriculture, and capital. This sort of organization allows Davis to develop selected illustrations of economic change in satisfying detail in spite of the relatively small size of the book: examples are the sugar industry, Amsterdam in the seventeenth century, and the economic decline of Spain.

Perhaps the outstanding achievement of this text is its incorporation of so many relatively recent or "post-Heaton" findings by specialists. There must be scores of such improvements; I gave up counting after the first two dozen. On the other hand certain minor aspects of the work are regrettable. The index is barely adequate; the five sketch maps are not worth the space they occupy; and the bibliography will please no one, since it is too short for advanced scholars and too concerned with French, German, Spanish, and Dutch items for beginners. Changes in the organization of manufacturing, so important in the older histories, are treated skimpily; economic policy and theory, treated

by some of us under the heading of "mercantilism" (but decidedly not by Professor Davis, who shuns the word), has just about disappeared from view.

Those who expect to find here a bold new synthesis will be disappointed. Professor Davis is interested in middle-range generalizations only. He uses a rather tenuous thread on which to hang his copious and valuable material: comparative economic development, or rather an unweighted listing of reasonable-sounding explanations for Iberian decline, Dutch stagnation, and English industrialization. For Professor Davis the search for general patterns, so dear to the hearts of the model builders, is of little importance. He is much more interested in divergence. He is quite serious in his use of the plural in the last word in his title. And indeed this may be the only sensible course to follow until the Braudelians on the Continent and the neoinstitutionalists in America turn up something more convincing in the way of a general theory of early modern economic history than is now the case.

MARTIN WOLFE

University of Pennsylvania

PIERRE LÉON *et al.*, editors. *L'industrialisation en Europe au XIX^e siècle: Cartographie et typologie* (Lyon, 7-10 octobre 1970). (Colloques internationaux du Centre National de la Recherche Scientifique. Sciences humaines.) Paris: the Centre. 1972. Pp. 619. 128. 40 fr.

This colloquium volume, lavishly produced, contains contributions by scholars—many of first-rank reputation—from numerous non-Communist European countries. They are written in five languages, with French predominant and English and German widely used. Rondo Cameron was the only American participant, but frequent reference was made to methodological advances in American economic history and some useful analogies drawn to the European experience. The conference had a distinct comparative flavor within the European context. Discussion after the major papers were delivered brought out some contrasts in national approaches, though at times falling flat, as is so common in such gatherings, when discussants took the line: "how interesting, and here is what happened in the region I am familiar with." Generally the French were most con-

cerned with structural factors, even in non-quantitative papers, the English and Germans with a range including more intangibles.

The variety of subject matter and quality defies summary in a brief review. Cartography receives little attention, and that rather antiquarian, despite the subtitle. The remainder of the volume is idiosyncratically organized. Essays on demography or banking, for example, pop up at widely separate points. Many of the essays on the smaller countries are weak, involving purely descriptive material; a long quantitative study on Belgium, with many careful graphs, tells us essentially that the Belgian economy was different in 1900 from what it had been in 1800. Other essays provide excessively familiar summaries, such as several long discussions of manufacturing up to the end of the eighteenth century. Treatment of demography, including an article by Armengaud, is disappointing, reminding us of how little class-specific material has been developed for the nineteenth century.

But there are contributions of importance. Several monographic essays, including two on Swiss textiles, contain useful data, but might better have been placed in a more specialized collection. At least two essays, though not novel, provide useful summaries of major developments: Mathias on Britain's uniqueness and Crisp on Russian industrialization. Jean Bouvier provides a neat typology of the ambiguous relationship between banking and industrialization. A quantitative essay on French railroads is splendid, involving much new material. In general those essays that really do focus on typologies, including regional studies of Normandy, Wales, and the Alpine area, hit the mark well. Particularly important is the explicitly comparative work of Wolfram Fischer, isolating the cases of Saxon and Westphalian development. Generally the better essays lead, as concluding remarks suggest, to a sense of regional (not national) typology based to an important degree on preindustrial differentiation. It might have been desirable to concentrate on these essays, producing a volume less commemorative and more accessible, even at cost to some *amours-propres*.

The most interesting conceptual problem not raised by the volume as a whole concerns its chronology. Participants were quite ready to

move into prenineteenth-century materials as background. But despite wide recognition of industrialization as a process, not an event, there was a temptation to see it magically stopping in 1900 or 1914. An essay by Postan raises important questions about the applicability of the English industrial model to twentieth-century industrialization in underdeveloped countries, but this still does not challenge what is essentially an English-based chronology for Europe. Hence the most stimulating essay of all, in terms of raising new questions, is one by Hartwell that discusses the development of the tertiary sector in industrialization; frankly recognizing the limitations of existing knowledge, the essay sees industrialization in twentieth- as well as nineteenth-century terms, talking about what it was becoming as well as what it briefly was. Here, clearly, is an approach that grows increasingly necessary if we are to understand industrial society as an ongoing phenomenon.

PETER N. STEARNS

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DOUGLAS H. ROBINSON. *Giants in the Sky: A History of the Rigid Airship*. Seattle: University of Washington Press. 1973. Pp. xxix, 376. \$15.00.

Airships are classified according to three structural types: nonrigid, semirigid, and rigid. Because of their reliance upon a pressure differential for the maintenance of envelope rigidity monrigids and semirigids are also known as pressure airships. This distinguishes them from the rigid airship, the hull of which, consisting of a rigid structural frame, maintains its shape irrespective of internal gas pressures. *Giants in the Sky* is the first scholarly study of the latter type and covers the period 1900-40.

A hundred and sixty-one rigid airships were built and flown, and according to the author, "all but two—which were metal clad—shared fundamental features evolved by the original inventor, Count Ferdinand Von Zeppelin." Because of General Zeppelin's experiments and the activities of the Deutsche Luftschiffahrts-Aktien-Gesellschaft, commonly known as Delag, Germany was to dominate the rigid airship industry. Some ten thousand paying passengers were transported by the Delag Airline during the 1910-14 period, and in the 1930s Germany developed a profitable trans-Atlantic service.

Numerous rigids were used by Germany during the First World War. Although the Zeppelin-type airship had great bomb carrying capacity its raids over England proved something less than a complete success. Not only was it vulnerable to attack from pursuit planes, but it also frequently became a victim of turbulent weather conditions and engine failure. Great Britain and France made small use of the rigid airship during the war and contributed little to its development.

Even though such famous U. S. Navy airships as the *Shenandoah*, *Los Angeles*, *Akron*, and *Macon* were copies of the German Zeppelin (the *Los Angeles* was actually built in Germany) the author has entitled one of his chapters "The Innovators: American Military Rigids." Indeed the United States did pioneer in the use of nonflammable helium gas instead of the somewhat lighter but dangerous hydrogen used in Europe. Also the Americans experimented successfully with the "hook on" method of carrying fighter planes and developed more effective mooring systems.

Perhaps the most interesting section of the book deals with the commercial operations of the *Graf Zeppelin* and the *Hindenburg* (over eight hundred feet long with a gas volume of some 7 million cubic feet) that were successful until the disastrous burning of the latter at Lakehurst, New Jersey, on May 6, 1937.

Airship buffs and aeronautical engineers will find *Giants in the Sky* a gold mine of information. The casual reader, however, may be overwhelmed by the clutter of material on dimension, gas displacement, and construction details that fill almost every page of the volume. Many historians will be disappointed in Robinson's failure to place the rigid airship in its proper perspective in relation to the overall military activities in World War I.

JAMES J. HUDSON
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MONTE DUANE WRIGHT. *Most Probable Position: A History of Aerial Navigation to 1941*. Lawrence: University Press of Kansas. 1972. Pp. xi, 280. \$13.50.

Monte D. Wright, an air force lieutenant colonel and a Duke Ph.D., has had the benefit

of personal experience as well as of scholarly inquiry in the subject of this book. This is a history of aerial navigation in the strict sense, that is, of the development of instruments and methods for determining the "most probable position" of an aircraft with respect to the earth's surface. It covers the whole range from the birth of aviation to World War II, from the problem simply of finding one's way home in a flight of a few minutes or hours to the finding of military targets for bombers in World War I, and from the historic transoceanic and polar flights of the 1920s to the round-the-world flights of the thirties, and the long-range bombing of the late thirties and early forties. Although the book claims coverage only to 1941 the author cannot escape inevitable references to the World War II experience.

The story of the rise of aviation always has been a glamorous one, but a technical accounting of the problems of navigation might appear to many to be its least glamorous side. Certainly it is one that, as a whole, has been most neglected. Colonel Wright has been able to overcome the deficiency without loss of the glamour in the tale. While he writes of technical instruments and methods the human interest stories of the men and women who used them comes through. True to the instincts of a good teacher (he is associate professor of history at the Air Force Academy) he introduces his subject by providing a background explanation of marine navigation as applied before World War I and then shows how the free balloon introduced the third dimension that characterizes aerial navigation.

The development of airplanes and dirigibles before and during World War I brought along the necessity of improved methods for navigation. Both the German and British forces used radio direction devices, but often with error and ill effects. While further improvement in radio navigation accompanied the establishment of overland airlines after the war, celestial navigation—long the system of reliance for marine navigation—reached new levels of utility with the development of transoceanic airlines. With the bubble sextant and artificial horizon, and with aircraft such as Pan American's Boeing Clippers fitted with astrodomes for celestial observations, navigators were able to correct their dead-reckoning estimates with great accuracy.

Colonel Wright's method is to explain each new navigational device within his general chronological approach and then show how it was applied on historic or pioneering flights. Thus Commander Richard E. Byrd reported to the Navy Department in 1925: "Sun compass very good when sun is visible" (p. 139). Lindbergh had great praise for the earth inductor compass that he used on his trans-Atlantic flight in 1927, though actually it was less reliable than he at first indicated. It failed Chamberlin when he tried for Berlin that year, and it failed Byrd in the *America* over the French coast, but it served well for Kingsford-Smith and Ulm on their great flight in 1928 from San Francisco to Australia via Hawaii and Suva.

There is no general statement of conclusions, but the book does end with a pointed reference to the limitation on the effectiveness of strategic bombing during World War II: "The air commanders knew that a B-17 crew could find a precise target, given clear weather and daylight operations. They believed, rightly, that a bombardment campaign of sufficient intensity could produce significant military results. They also knew what flying weather over central Europe is like. The World War I experience was unambiguous. Commercial aviation had compiled additional meteorological data between the wars. The commanders did not see all those facts and assumptions together, and no one pointed with 'an insistence compelling action' to the central contradiction: that there would not be enough clear days to execute the massive campaign, that weather forecasts would be fallible, and that bombing through clouds and industrial haze would be necessary."

It is an accomplishment for a military man to escape the military language of the Pentagon for direct, active, clear, and concise English, or for the technician to explain technical matters in a way that holds the interest of the layman. It may be even more of an accomplishment for one to escape the pedantic and tortured prose of a doctoral dissertation. Wright has done all these.

The book includes the British, French, and German experience as well as the American. Extensive notes and an impressive bibliography testify to the scholarship. Numerous drawings

and photographs add to the clarity of the text. It is a work well done.

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MELCHIOR PALYI. *The Twilight of Gold, 1914-1936: Myths and Realities*. Foreword by DONALD L. KEMMERER. Chicago: Henry Regnery Company. 1972. Pp. xxiv, 365. \$15.00.

The late Melchior Palyi has written another history of the demise of the gold standard, and he clearly regards its downfall as a tragedy for the world. Monetary policy can stabilize either the domestic price level or the price of gold (foreign exchange rate), but not both. Adherence to the gold standard leaves domestic prices to be determined by foreign circumstances, and Palyi prefers this external stability to the goal of domestic stability that is emphasized by modern policy makers. The gold standard is preferred for two reasons. First, it is economically efficient because it promotes international mobility of goods and factors of production. Second, it protects the liberty of citizens from arbitrary taxation by imposing a monetary constitution on the government.

Palyi's distrust of government does not extend to central bankers, whom he regards as guardians of the soundness of money. His prototype of the pragmatic, responsible central banker is Montagu Norman of the Bank of England, and he sharply distinguishes the independent, apolitical character of Norman from the vacillation of elected officials who yield to political pressure. He regards the floating of the pound in 1931 as a tragedy from which sterling never recovered, and it occurred only because Norman was too ill to protest the action.

The book expresses a curious commitment to private markets, since it is critical of their ability to cope with uncertainty through such arrangements as insurance and futures contracts. Speculation is viewed not as a useful bearing of risk that stabilizes prices around their trend, but rather as antisocial mischief that must be controlled. Speculation has damaging effects on real estate and stock prices, and especially on foreign exchange markets, and it is concluded that destabilizing international-capital flows made the inter-war experiment

with floating exchange rates a dismal failure.

As a practical man Palyi is suspicious of theory. Prices did not fall in the 1930s because of the sharp contraction of the money supply from 1929–33. The quantity theory of money is too simple. The Federal Reserve could not prevent the contraction since they could not force people to spend. Churchill was not responsible for economic stagnation in Britain by overvaluing sterling in 1925. Purchasing power parity calculations of the exchange rate are too simple.

Palyi is short on alternative explanations, but in general the problems of the 1930s were caused by the denial of discipline as exemplified by private speculation, deliberately unbalanced budgets, and a lack of commitment to gold. There is little new information in Palyi's chronicle, but the style is fluid and the anecdotes involving central bankers are entertaining.

THOMAS GRENNES

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MICHAEL G. FRY. *Illusions of Security: North Atlantic Diplomacy, 1918–22*. [Toronto:] University of Toronto Press. 1972. Pp. xii, 221. \$12.50.

A considerable part of this succinct and highly significant book on post-World War I diplomacy is devoted to the concept of Atlanticism. Viewed by its proponents as a panacea for the major world problems, the idea prevailed well before 1914 but exercised a special fascination over some members of the British Empire elite following World War I. While that same elite also included men who were skeptical and even hostile to the Atlanticists, the latter were well entrenched in the Foreign Office, the Board of Trade, the diplomatic service and throughout the Dominions. Essentially the Atlanticists perceived the peace and hope of the world as hanging upon Anglo-American cooperation in concert with Canada, whose subordinate role in the alliance would be enhanced by Ottawa's potential service as mediator between Washington and London.

Because Atlanticists like Auckland Geddes, Lord Milner, and Arthur Balfour differed in their respective outlooks a general profile of Atlanticism would necessarily contain a number of inconsistencies. Milner's and Philip

Kerr's missionary idealism was, however, typical of most Atlanticists. They saw the Commonwealth as a model in which white, Christian, advanced peoples controlled and guided backward races toward higher values and self-government. A fusion of cultural concepts was neither sought nor desired by the English-speaking imperialists. They aspired instead to disseminate those "superior" Anglo-Saxon political and spiritual ideals that, when universally embraced, would afford a self-perpetuating peace for all mankind. Several factors obstructed the Anglo-American harmony Atlanticists sought to promote during the 1919–22 period: President Wilson's personal and political collapse; United States isolationism and attitudes toward war debts; Middle East tensions; and the question of naval power for imperial defense, a matter which possibly touched England's rawest nerve. Fry convincingly argues that, despite the achievements of the Washington Naval Conference and a few other modest gains, the ultimate failure of the Atlanticists lay in their excessive and unrealistic expectations. Even less ambitious efforts at altering global policies, he reasons, might have enjoyed little success during a period of such intensive reconstruction.

The paraphrases of British ministerial reports, which comprise a substantial portion of the author's account, yield a somewhat mixed result: while the witticisms are engaging and the commentary revelatory of the basic objectives of the empire elitists the reader must contend with a distracting measure of superfluous detail in a number of loosely edited and wide-ranging dispatches. Perhaps the most serious limitation of this otherwise excellent study is the paucity of American source materials. We are told that United States Ambassador George Harvey warned British Foreign Secretary Lord Curzon of the further deterioration of Anglo-American relations in 1922; how Washington felt that Great Britain and Japan had somehow outwitted the United States at the Washington Naval Conference; and that the resolution of the Irish question had a salutary effect upon the overall state of British-American affairs. In each of these instances, however, English or Canadian sources are cited; only occasionally do a few published State Department papers appear as supportive evidence. The views of

American Atlanticists are largely ignored.

None of this seriously detracts from the book, and Fry demonstrates a thorough familiarity with the published and archival sources for his period. He possesses an impressive knowledge of the principal personalities of the era, more notably the empire elitists, whose strengths, weaknesses, and influence in the articulation of policy are ably perceived and assessed. The author concludes that the Atlanticists, in their mistaken belief that Great Britain and the United States shared common global purposes and goals, reached for an allusion after World War I. Fry suggests, possibly for the consideration of some future study, that this failure in perception did not prevent others from resurrecting the discredited Atlanticist theme after World War II. This monograph richly deserves a place on the selective shelf of scholarly studies in twentieth-century diplomacy.

THOMAS E. HACHEY
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HOWARD M. SACHAR. *Europe Leaves the Middle East, 1936-1954*. With an introduction by WILLIAM L. LANGER. New York: Alfred A. Knopf. 1972. Pp. xviii, 687, xxxviii. \$15.00.

In his new book Professor Sachar has examined the political and military machinations of those European powers—namely Britain, France, Italy, Germany, and Russia—for whom the Middle East was a major arena of conflicting interests and the process by which those powers withdrew from the region in the years between 1936 and 1954. It will come as no surprise that a considerable portion of Sachar's attention is, of necessity, devoted to the problem of Palestine.

This is largely a descriptive work, comprehensive in scope, well organized, and written in a clear and interesting style. While Sachar empirically offers little that is unknown or original, he has undertaken a problem worthy of consideration. It is a book also useful for the quantity of information contained therein, which is presented with care, accuracy, intelligence, and some insight. The maps are clear and supportive of the text; the bibliography is helpful, and the index usable. Here is an instance where the reader can safely accept the author's work but need not necessarily accept his ideas or conclusions.

A few tremolos of doubt are set in motion by Sachar's premise that Europe's "withdrawal was to be far more complicated and invidious an ordeal than the initial, rather straightforward and simplistic military conquest" (p. xvi). Then the first chapter commences with the assertion that after a half millennium of Islamic fatalism and stagnation in the Middle East, "the vanguard of progress was the Anglo-Indian infantry who marched into Baghdad in late 1917, the Anzac cavalry who stormed across Palestine in 1918, the French marines who disembarked at Beirut harbor in the last weeks before the Armistice" (p. 5). Further doubts are raised by Sachar's tendency merely to describe rather than to explain or analyze, as he does for other major parties concerned, the motives and attitudes of the Arabs toward various issues (especially chs. 1-4) and to place too much emphasis on xenophobia to characterize Arab responses to European domination (pp. xvii, 35, 56, 163, 331, 404, 554, 613). Finally, the author concludes that "even the most enlightened of Western efforts to modernize and liberalize the Middle East foundered on the rock of Moslem immobility" (p. 609).

Surely the conflicting nationalist impulses that propelled Europe's expansion into the Middle East were at least as "complicated and invidious" as the process of withdrawal to which they were integral. And what ever happened to the preceding century of Ottoman and Arab reforms, both Muslim and secular, which in many basic ways changed and updated Middle Eastern societies and institutions? The fact that the Europeans occupied portions of the Middle East not primarily to "modernize and liberalize" but to serve their own economic and political interests, and often to discourage indigenous efforts that might have led to radical changes in the status quo, should be given at least equal causal weight with "Moslem immobility," even allowing for the imprecision and stereotypical notions implicit in that phrase. In the last analysis, it was because the Europeans occupied their lands and controlled their destinies that the Arabs opposed them.

Professor Langer in his introduction has unwittingly done a disservice to Sachar and the reader by insisting on the "objectivity" of this book. While Sachar has obviously made every effort to show all sides of the issues involved

and has distributed his criticisms freely, nevertheless the ambience of the book is Western and pro-Zionist (but not without understanding for the Arab view). The Arab uprisings are consistently described in terroristic terms, while the Haganah and Irgun are labeled as "flying columns" and "mobile commando groups" and their actions are designated as "active defense" (pp. 90-93). Haj Amin was "notorious for his deviousness," but no mention is made of the devious tactics of Chaim Weizmann or Ben-Gurion. British appeasement of the Arabs, Sachar states (p. 99), was to be at Jewish expense. But, to the Arabs, was not every instance of appeasement of the Jews made at the expense of the Palestinians?

The point here is that although Sachar has given a particular coloration to his book, because he has at the same time made a sincere and largely successful effort to present a balanced account of the facts and events, he has produced something more interesting and honest than the "objectivity" claimed for him by Langer. Despite these criticisms, this book is welcomed both for the wealth of its information and for Sachar's undertaking of a complex problem.

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JOSEF KORBEL. *Détente in Europe: Real or Imaginary?* Princeton: Princeton University Press. 1972. Pp. viii, 302. \$10.00.

This new book by the well-known specialist in international affairs dwells on the present state of affairs between East and West. The key word is détente, and Korbél dates its beginnings in the 1960s. Examining ideological, political, and economic aspects of this relaxation of tensions the author concludes that "the détente is real, although limited in nature and scope, but its durability is at best uncertain and at worst illusory." I find myself in agreement with this formula as well as with Korbél's final conclusion that real cooperation could only replace the present vague détente after an internal liberalization of the Soviet Union. No less a person than the famous Soviet scientist Sakharov recently stressed this very point.

Korbél's statement that "modern history has demonstrated the inability of a democratic

country successfully to infiltrate and subvert a totalitarian regime," may be understood to mean that the rest of the world is condemned to the role of a mere spectator of evolutionary processes in the Communist bloc. But the author is apparently warning here against facile beliefs in the efficacy of propaganda without necessarily negating the usefulness of subtler means of influencing the East.

The main value of Korbél's study, especially to a historian, lies in showing that détente means different things to various powers, particularly the West European states. Like the "spirit of Locarno" the term lends itself to divergent interpretations. The Soviet Union regards it largely as a means to achieve an international legal confirmation of the status quo in East Central Europe and to diminish American influence in Europe. Britain sees it in terms of accommodation and normalization of relations between the two parts of Europe. It is a low-key approach, and British involvement in East Central Europe appears minimal. France, during the de Gaulle period, promoted an ambitious program expressed through the slogan "détente to entente to cooperation." In the name of France's grandeur, Paris sought to achieve some restraints over West Germany, an extension of French influences to the states of East Central Europe, and a minimization of the American influence on the European continent. This policy produced few results, and from 1970 the focus of détente policies shifted to Bonn. Brandt's *Ostpolitik*, to which Korbél devotes a good part of his book, is represented as pursuing the solution of the problem of a divided Germany and establishing on a new footing the relations between Germany and her Eastern neighbors.

In reading Korbél's discussion of the policies of the three West European states I had occasionally the feeling that *plus ça change plus c'est la même chose*. Britain's relative aloofness to East Central Europe; France's vigorous diplomacy unaccompanied by economic achievements; German presence, at least economic, in Eastern Europe; Western fascination with the Russian market—surely we have seen all this before. But Korbél does not pursue these interesting historical parallels. His objective is to examine the present, and his thesis that the détente is a phase in the East-West relations, although a welcomed one, and not a panacea,

seems to be borne out by events that have taken place after the publication of his book.

PIOTR S. WANDYCZ
Yale University

ANCIENT

FRANK MOORE CROSS. *Canaanite Myth and Hebrew Epic: Essays in the History of the Religion of Israel*. Cambridge, Mass.: Harvard University Press. 1973. Pp. xviii, 376. \$14.00.

These essays reflect the impact of excavated inscriptions on biblical religion and institutions. The most important category of such inscriptions, which has revolutionized Old Testament studies, is from Ugarit. A better-publicized group of such texts is the collection of Dead Sea Scrolls from Qumran, a small archeological site whose early history is the topic of the final chapter.

This book deals with various aspects of Israelite religion against the background of newly found material, for example, concepts and names of the gods, cult, the tribal league and monarchy, prophecy, covenant, and literary criticism ("Documentary Hypothesis") of the Old Testament. The latter continues to exert so great a force on biblical historiography that it merits careful scrutiny.

The essence of the documentary hypothesis is that the Pentateuch has been compiled by a redactor from a number of unknown authors, notably "E" (who calls God Elohim), "J" (who calls Him Jahweh), D (whose input is Deuteronomic), and P (whose orientation is Priestly). The extent and dates of the J, E, D, and P documents are disputed, but the system is in general accepted.

The first creation (Gen. 1:1-2:3), in which God is called Elohim, used to be attributed to E; the second creation (Gen. 2:4-3:24), in which God is called Jahweh-Elohim, used to be attributed to JE because presumably J and E authorship had been combined.

The fact is that Genesis 1-3 portrays successive and interrelated creations: the first transitory one by Elohim and the second permanent one by his son Jahweh-Elohim. In Ugaritic El (Elohim) is the father of Yw-El (Jahweh-Elohim). Proverbs 30:4 alludes to the two crea-

tors as father and son. In the opening chapter of John, the Son is referred to as a creator.

Precisely what the author of this book means (on p. 303) by attributing parts of Genesis to JE is not clear. That the Pentateuch embodies earlier sources is certain; that any source such as JE ever existed is another matter.

On page 219 the office of "judge" ("ruler") is described as "charismatic." Charisma (divine inspiration) was necessary but not sufficient for qualifying the rulers known as the Judges. The latter had also to belong to the ruling aristocracy at least on their fathers' side. The candidate could be the youngest son of a small clan from the least of the noble tribes, but he had to be sired by a member of the aristocracy—or accepted as such.

CYRUS H. GORDON
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GEORGES DUMÉZIL. *From Myth to Fiction: The Saga of Hadingus*. Translated by DEREK COLTMAN. Chicago: University of Chicago Press. 1973. Pp. xii, 253. \$12.50.

GEORGES DUMÉZIL. *Heur et malheur du guerrier: Aspects mythiques de la fonction guerrière chez les Indo-Européens*. ("Collection Hier.") Paris: Presses Universitaires de France. 1969. Pp. 148. 20 fr.

GEORGES DUMÉZIL. *The Destiny of the Warrior*. Translated by ALF HILTEBEITEL. Chicago: University of Chicago Press. 1970. Pp. xv, 168. \$8.75.

GEORGES DUMÉZIL. *The Destiny of a King*. Translated by ALF HILTEBEITEL. Chicago: University of Chicago Press. 1973. Pp. 155. \$10.00.

For the past twenty-five years Georges Dumézil has been professor of Indo-European civilization at the Collège de France. With massive erudition and in rapid succession he presents us with a number of books translated and revised from his previous works. In spite of their seeming diversity, they all deal with his favorite theme, a comparison of the mythology of the various Indo-European peoples. His main theses, recently made available to the English-speaking world through the translation of *Archaic Roman Religion* (AHR, 77 [1972]: 1099-1100), is that the Indo-European peoples divided life into three functions—sovereignty, physical force, and fecundity. These functions can be found distributed among three or more

mythological personages such as the Vedic Mitra-Varuna, Indra, and the Nāsatya twins, and the Nordic Odin, Thor, and Freyr, or among legendary human kings, such as the Roman Romulus-Numa Pompilius, Tullus Hostilius, and Ancus Marcius. Dumézil is convinced of a "conscious authorship" responsible for the transposition of archaic mythology to legendary human history, but in most cases we are too far removed from the authors to perceive this transposition. The only exception is the Scandinavian world. Here we have preserved, on one hand, the Eddas and the Sagas, which retain much of the original mythology, and, on the other, the works of Snorri Sturluson in Iceland and Saxo Grammaticus in Denmark who transposed this mythology into purported history. Since Dumézil started his comparative research with a study of Scandinavian mythology (*Mythes et Dieux des Germains* [1939]), his views on the Scandinavian problems are particularly interesting and well founded. He has dealt with the authorship of Snorri in *Loki* (1948) of which he promises a radically revised edition. *From Myth to Fiction* deals with Saxo Grammaticus, the Danish medieval historian. The first half of the book contains a long essay analyzing Saxo's "Saga of Hadingus," first published in 1953 from lectures given at the Collège de France. The second half consists of appendixes of which all but the last deal with various aspects of Saxo's technique in transforming mythological material into "history." *From Myth to Fiction*, then, is a study of Saxo's literary procedures. That neither the title nor the subtitle makes this clear is probably due to the fact that in 1953 Dumézil kept the possibility open that the alterations made in Saxo's version were not his own but were derived from an unknown Icelandic source. His later studies leave no room for this hypothesis, and he now seems convinced that Saxo is directly responsible for the changes. Thus the transposition from religion to history that is beyond the pale of historical investigation elsewhere in Indo-European civilizations can be examined closely in the Nordic context. The story of Hadingus is found in the beginning of Saxo's *Gesta Danorum* where he is depicted as an early Danish king and a mighty warrior, a worthy predecessor to Saxo's own king Waldemar the Victorious. This idea has

generally been accepted in modern scholarship, but Dumézil returns to an earlier view that pointed to analogies between the saga of Hadingus and the myth of the god Njörðr. Beyond these Dumézil is able to show that in fact the career of Hadingus generally follows that of Njörðr with a clear division between two periods, one representing the function of sovereignty and the other that of fecundity. In the appendixes he treats other mythological material transformed by Saxo, for example, how the mild and peaceloving Baldr of myth was changed to Saxo's belligerent and jealous Balderus. Dumézil concludes that Saxo, indifferent to the former mythological value of the stories, took them apart and changed them according to his literary whim, while still leaving the mythological structure intact. The translation by Derek Coltman is done well, but Adam de Brême (p. 52) and Adam Bremensis (p. 86) should be Adam of Bremen.

Heur et malheur du guerrier, appearing simultaneously as *The Destiny of the Warrior*, is largely an updating of a previous work from 1956. The revisions have been done "in an effort to prepare for the inevitable autopsy as proper a cadaver as possible, that is, to deliver to the critic of the near future, in an organized and improved form, the results of the endeavors, of varying success, carried out over the past thirty years" (p. xiv). The subject of his examination is the function of force, and he shows in the first part of the book the similarities between the Indian warrior-god Indra and the legendary Roman warrior-king Tullus Hostilius. The second part focuses on the unique solitude of the warrior type, who represents force and opposes the representatives of the two other functions of sovereignty and fecundity, who often appear in pairs. Solitude means freedom that may lead to transgression. Dumézil shows how the theme of three successive sins committed by the warrior is found in the Indian god Indra, the Scandinavian Viking Starcathe-rus, and the Greek hero Heracles. The last and weakest part of the book clarifies some of Indra's exploits by drawing parallels with scenes from Iranian, Armenian, Scandinavian, Irish, and American Indian mythology and folklore.

The Destiny of a King, translated from the French edition of 1971, publishes the Haskell

Lectures at the University of Chicago. Of interest mainly to mythographers, this book contrasts the Iranian legendary king Yima with his three counterparts in Indian mythology—Yama, Yayāti, and Vasu Uparicara. The career of Yayāti clearly illustrates the author's trifunctional structure, and a comparison between Yayāti's daughter Mādhavī and the Irish queen Medb convincingly shows similarities within the vast Indo-European family. The book ends with some fascinating ideas on the phenomenon that kings often surrounded themselves with virgins to protect their kingship.

JENNY JOCHENS
Towson State College

M. I. FINLEY. *The Ancient Economy*. (Sather Classical Lectures, volume 43.) Berkeley and Los Angeles: University of California Press. 1973. Pp. 222. \$8.95.

WILLIAM I. DAVISSON and JAMES E. HARPER. *European Economic History*. Volume 1, *The Ancient World*. New York: Appleton-Century-Crofts. 1972. Pp. xiv, 288. \$8.95.

In *The Ancient Economy* M. I. Finley brings together significant conclusions and insights he has put forth in individual studies over the past decades, adds a number that are new, and presents them all with the lucidity, trenchant style, and probing analysis that is the hallmark of his work.

Finley mercilessly delivers the *coup de grâce* to many a sacred cow that still disfigures current text and reference books. An initial chapter on social and economic class and status contains a discussion that should once and for all end talk of a clash of interest between the two great groups of Roman rich, the senators and the equites, the one being viewed as landholders and the others as moneyed interests; both were landholders, and they cooperated happily and profitably in financial ventures. Chapter 2, on slavery (and perhaps the highlight of the volume), points out hitherto unrecognized features revealed by recent scholarship, much of it Finley's own: that many slaves—for example, Roman slaves who had a *peculium*, property of their own—were hardly slaves at all; that in the overall picture, other forms of dependent labor—debt-bondage, clientage, helotage—were at most times as important as chattel slavery and at certain times

more important: that there was never a significant free labor force, hence all argument about slave labor driving out free is pointless and all conjecture about their comparative costs meaningless; that Roman Stoicism and Christianity, contrary to what is so often said, had scant effect on the decline of slavery. A chapter on landlords and peasants emphasizes the overwhelming importance of agriculture in the ancient economy and demonstrates that the ancient attitude toward landholding was based on social and psychological values far more than economic: people held on to land for emotional reasons and acquired it for status; they rarely if ever looked for the maximum profit that could be wrung from it. Indeed, the whole thrust of Finley's book is to make patently clear that in ancient times both individuals and states thought primarily in political, social, and psychological terms and only secondarily in economic.

There are areas where Finley seems to press his views too far. In emphasizing agriculture he rightly scales down the importance of manufacture—yet perhaps more than is called for. There is a significant new form of evidence that he does not cite, the cargoes found in ancient wrecks. At least three have been found loaded with thousands of roof tiles, another with thousands of lamps, yet another was carrying hand grain mills—all this may point to a volume of manufacture and movement of its products beyond what Finley cares to allow. Again, in emphasizing, and rightly so, the basic self-sufficiency of most ancient communities, he sets stringent limits (compare p. 165) to the nature and extent of seaborne trade. For example, he limits the Greek traffic in wine to famed regional vintages, asserting that *vin ordinaire* was normally produced at home (p. 133). Yet, as the ubiquitous Cnidian and Rhodian shipping jars reveal, it went wherever nothing better was available, and that amounted to no negligible volume.

Finley has given us an important and provocative book. It is a pity that it did not appear in time to be digested by the authors of volume one, *The Ancient World*, in a new series on Europe's economic history. For Davison and Harper repeat much of the time-honored doctrine that Finley has so effectively demolished. They have, as a matter of fact, dared

to tread the length and breadth of a highly controversial field relying almost wholly on secondary sources, including some that are mere popular works or undergraduate texts. In order to set the scene of their story, they provide rapid surveys of political events that all too often come out as indigestible globs of names, dates, and actions (compare pp. 74-75).

All this is too bad since the authors, when they can leave the trees of their narrative and survey the forest, reveal clear and accurate vision and arrive at conclusions close to those reached by Finley, as in their review of the role of commerce and manufacturing in the economy of the Roman Empire (pp. 209-14) or their final chapter summarizing the limitations of the ancient economic system (pp. 235-40). But these bright spots are few and far apart; in between lie a welter of highly questionable statements, misunderstandings (compare, for example, their remarks on Greek economic thought [pp. 122-28] with Finley's [pp. 19-21]), exaggerations (for example, on the economic aspect of the Peloponnesian War [p. 127]), and, inevitably, uncritical use of sources (compare p. 184 n.24, with Finley, p. 183 n.52, for a glaring example).

LIONEL CASSON
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MOLLY MILLER. *The Thalassocracies*. (Studies in Chronography, 2.) Albany: State University of New York Press. 1971. Pp. xii, 185. \$10.00.

Along the lines of earlier researches published in *The Sicilian Colony Dates* (1970), Dr. Miller here takes up the problems of chronology presented by the ancient registers in which were recorded the traditional dates of the sea powers that succeeded one another in the Greek world from the Trojan War to the Persian Wars. The concept of thalassocracy, Dr. Miller believes, represents a principle of universal history in the Greek world that will provide a truer picture of early Greek history than the established tradition of the development of the city-state or the sum of local histories. In addition to being incomplete and in part damaged, our surviving records of the early chronographic traditions present difficulties because the compilers arranged events in periods which prove to be groupings that are based on dif-

ferent methods of reckoning. These periods comprise either twenty-seven or thirty-nine years, so that according to the chronographic conventions employed in different traditions, the duration of individual thalassocracies may vary.

Dr. Miller undertakes to correct these discrepancies and to synchronize the entries in the belief that an original "Register of Thalassocracies" may be visualized. The effort involves a formidable exercise in speculative arithmetic and number theory, combined with what are thought to be necessary restorations and rearrangements in the texts. It is assumed that the reader is a scholar who is at home in this method of research and who shares Dr. Miller's earnest conviction that the method is valid and that, by means of the ingenious manipulation of figures, it will produce acceptable conclusions. There will be others, however, who in spite of Dr. Miller's learning and enthusiasm will share the skepticism of scholars who have dealt with this problem, notably J. L. Myres (whose studies Dr. Miller cites, p. 2 n.4; p. 180). In any case some scholars will be dubious about a tradition of early Greek history that has to be reconstructed in this fashion.

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JEAN DES GAGNIERS. *L'Acropole d'Athènes*. Quebec: Presses de l'Université Laval; distrib. by International Scholarly Book Services, Portland, Ore. 1971. Pp. 108. \$9.95.

C. A. DOXIADIS. *Architectural Space in Ancient Greece*. Translated and edited by JAQUELINE TYRWHITT. Cambridge, Mass.: MIT Press. 1972. Pp. xxxvii, 184. \$12.50.

Jean des Gagniers's *L'Acropole d'Athènes* is a popular publication, fitted out with the usual encomiastic illustrations, indifferently chosen and uneven in quality, with a simplistic, gushy text unworthy of a university press, and clearly inflated to a trim size unjustified by substance. This volume purports to be an introduction to the history and development of the Athenian Acropolis from the earliest phases in the second millennium B.C. to modern times. It fails to achieve this purpose, being neither adequately

descriptive nor historically comprehensive, as references to John (not Jean) Travlos's *Pictorial Dictionary of Ancient Athens* (1971) and to the still vital but old book by Gerhart Rodenwaldt (not Rodenwalt) and Walter Hege on the Acropolis (1930) make all too evident. It is impossible to recommend des Gagniers's book to the general public as either a coffee-table treasury or a solid introduction to the monuments of the Acropolis because the volume so completely lacks both the comprehensive coverage and intellectual content already available in other publications that are similarly intended for the interested lay reader.

Very different, however, is the publication of C. A. Doxiadis's dissertation on ancient Greek site planning, which has now been translated from German and handsomely produced by the MIT Press. In 1937 Doxiadis—now certainly better known as an architect and regional planner and as the leader of a theoretical group concerned with the entire physical environment in which man functions (Ekistics)—undertook his challenging study of Greek architecture without limiting himself to the traditional examination of individual buildings, usually a temple, conceived as an independent architectural item. Instead, convinced by his theory that the Greeks disposed buildings in space according to principles of cognition, Doxiadis occupied himself with defining the spatial, visual, and mathematical organization of twenty-nine Greek sites, for the most part sacred precincts and, with the exception of Palmyra, located on mainland Greece or in the Greek towns of Sicily and western Asia Minor. He tried to expose the rational system of architectural siting in Greek design by which the ancient architect sought not only to create a coherent architectural ensemble within a site, but, also, to make that coherency visually and conceptually accessible to the observer from significant points of view determined largely by the placement of entrances, the temporal path of ritual, and an insistence upon the solid, three-dimensionality of architectural form.

Doxiadis presented his theory and its ramifications in a brief introduction, while the greater part of his study consisted of an empirical demonstration of the validity of that theory based on a careful examination of the

selected sites, using the best available archeological information and constructing illustrative plans and reconstruction. The basis of this theory was the discovery that the human viewpoint was the determining factor in Greek design and that point was established primarily at the place from which the site as a whole could be observed, usually the principal entrance. Subsequently the following principles were used: each important building was visible as a solid body from the viewpoint, because three corners were revealed, or the building was completely concealed; the radii from the vantage point that determined the position of these three corners formed certain specific angles, equal in size on each site, and these angles in turn fell into two categories, corresponding to a division of the total field of vision (a circle) into ten or twelve parts; the twelve-part division was used for the Doric style, the ten-part for the Ionic. Furthermore, the position of buildings within the site was determined not only by the angle of vision but also by their distance from the viewpoint, and those distances were based on simple geometric ratios derived from the angle of vision; one such angle, often in the center of the visual field, was left open as a "sacred way," usually oriented toward east or west according to the demands of the ritual. And last, the buildings were frequently disposed so as to incorporate or accentuate features of the landscape and thus develop a unified (natural) composition.

Although the rational and humanistic tendencies of Greek thought have long been recognized, Doxiadis's attempt to demonstrate that Greek artists wanted to objectify the architectural experience through a comprehensive system whereby the constituent buildings on a site, the site itself, and the surrounding landscape could be brought into a stereometric whole, has met with neither widespread response nor acceptance. To many archeologists and classicists his ideas were controversial and unproved, lacking admittedly any explicit textual basis in Greek literature, although Doxiadis influenced R. D. Martienssen, *The Idea of Space in Greek Architecture* (1956), and especially V. J. Scully, Jr., *The Earth, the Temple, and the Gods. Greek Sacred Architecture* (1962). Perhaps his is a still precocious idea of general as well as specific applicability, and the

MIT Press is to be complimented on the republication of this important treatise. Correctly, the editor preserved the original text without revision and thereby fixed its place in the history of ideas and of classical and archeological scholarship. Nevertheless, although a few, inadequate citations to work on Doxiadis's sites after 1937 were added in this edition, it would have been very useful to list, and possibly even summarize, the principal reviews and characterize the reception this book has received in the past thirty-five years.

RICHARD BRILLIANT
Columbia University

N. G. L. HAMMOND. *Studies in Greek History: A Companion Volume to A History of Greece to 322 B.C.* New York: Oxford University Press. 1973. Pp. xvi, 574; 7 plates. \$40.00.

As the author explains in the preface, this book is a companion volume to his well-known textbook, *A History of Greece to 322 B.C.* It takes up selected controversial points in Greek history and is intended to help students by giving detailed evidence for, and reasons behind, conclusions presented in the textbook. The book is divided into sixteen chapters, treating subjects, arranged chronologically, beginning with the Mycenaean world and concluding with the Battle of Chaeronea. The first three chapters are concerned with the earliest periods of Greek history. They present essentially new material and are based on recent archeological discoveries. The remaining thirteen are articles published by the author between 1937 and 1968, revised and brought up to date for republication. The material is illustrated with twenty-three figures and maps of high quality and by seven pages of photographic plates, some of which, unfortunately, are unclear.

Discussion of subject matter will be restricted to the first three chapters, since the others develop viewpoints already expounded in the *History of Greece*. In treating the "Origins of Some Mycenaean Rulers" (chapter 1) the author notes the evolution of mortuary chambers into tholos tombs and a marked conservatism in burial practices at Mycenae in the period from around 1700 to 1300 B.C. He believes that such practices were not indigenous to Mycenaean Greece and that they were imported

by intruders. These, he thinks, came from central and southern Albania, spreading southward gradually in the Middle Helladic period. In a related chapter (chapter 2), "The Arrival of Greek Speech in the Southern Balkans," he develops the view that speakers of proto-Greek were present in Albania, southwest Macedonia, and Leukas around 2600 B.C. and that they evolved the dialects spoken in this historical period before penetrating the Greek peninsula beginning around 1900 B.C. Before the end of the Bronze Age, speakers of the Ionic and Achaean dialects were established in southern Hellas, those speaking Doric remaining in their original homeland, Epirus. In discussing "The Impulses which Started the Dorian Invasion" (chapter 3), the author argues that archeology and literary tradition suggest that the Dorian invasion of Greece began around 1140 to 1120 B.C. and that this, in turn, was caused by pressures on the Dorians by other peoples coming from the north.

These and other arguments are buttressed by meticulous scholarship characterized by a command over original sources and a painstaking attention to minutiae, which do not, of course, preclude disagreement with his conclusions on the part of other authorities. As might be expected, the general thrust and coloring of the author's *History of Greece* tend to be reflected in this companion volume. His main interests are clearly topographical and military, and he is at his best when reconstructing battles and bringing to bear his unexcelled firsthand acquaintance with the Greek countryside. His interests also affect the balance of topicality, eight of the chapters having a strong topographical or military orientation; only one, treating "Personal Freedom and its Limitations in Aeschylus' *Oresteia*," deals with a cultural subject.

This book will serve the specialist who seeks a handy, one-volume anthology of the more important papers of a distinguished Hellenist. Whether it will be very useful for students as a companion to a textbook of Greek history is another matter. The choice of topics is much too limited and even idiosyncratic to provide a reasonably wide range of collateral reading that students might expect, even though the author cannot be faulted for choosing to include in an anthology of his own articles those on topics to

which his contribution has been the most original. The high level of sophistication of the arguments in many cases also precludes their being of much help to the average American undergraduate, even if he has Greek.

WILLIAM G. SINNIGEN
Hunter College,
City University of New York

T. B. L. WEBSTER. *Athenian Culture and Society*. Berkeley and Los Angeles: University of California Press. 1973. Pp. 309. \$10.00.

The cultural achievement of classical Athens was firmly rooted in its political, economic, and social life and in the nature of the citizens it produced. To describe the manner in which Athenian poetry and drama, art and architecture, history and philosophy, and education and religion were created by and reacted upon the social fabric of a small society in the 150 years from the Persian Wars to Alexander is, however, no easy task. It is precisely this that T. B. L. Webster has attempted here.

Webster begins with a brief overview of political, economic, and religious life and then proceeds to a topical analysis of the Athenian cultural achievement. For him the relationship between culture and society is to be found by examining, first, the nature and competence of audience or patron and the social position of the practitioners of the art; second, the aim and achievements of the practitioners; and, finally, the growth of critical theory and the interchange of methods and ideas among the various arts. Within the boundaries thus set, Webster creates a number of interesting and valuable chapters, especially those on pottery, architecture, sculpture, and painting, drama, and geography, medicine, and history.

Webster's program is, however, limited. It tends to treat the "society" portion of the formula "culture and society" as if it consisted of nothing more than "audience" and patron in addition to some immediate political influences. This is apparent, for example, when Webster writes: "A small city with many meeting places, an open society, a viable social structure, intelligent patronage, educated audiences, the spirit of competition, these are the things that classical Athens offered to artists, poets, and thinkers, and they are partly responsible for

the flowering of culture" (p. 265). Surely there was a deeper relationship between culture and society than this would imply. The very categories of Athenian culture (drama, history, rhetoric, painted pottery, and so forth) are by no means obvious or necessary, and they clearly derive from the felt needs and requirements of social and economic life. To raise the question "Why does a society produce the particular cultural forms it does?" takes us to the heart of the culture-society relationship, and it is precisely there that Webster refuses to go.

This refusal is doubtless a product of Webster's shortcomings as a historian of political and economic life; the chapters on these topics are indeed inferior to those on art and literature, Webster's main interests. The book does not, therefore, offer quite as much as it initially promised. Nevertheless, Webster succeeds to a large extent in portraying the richness of Athenian culture in the refreshing perspective of its ancient audience, and the book is well worth reading for those (mainly nonspecialists) who wish to learn more about the civilization of classical Athens.

ROBERT A. PADGUG
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D. A. RUSSELL. *Plutarch*. (Classical Life and Letters.) New York: Charles Scribner's Sons. 1973. Pp. 183. \$8.95.

This is the latest in a series of books on Plutarch that has appeared in the last few years. The first three chapters contain general background information on Plutarch's life, career and family, his language and style, the extent of his reading, and the question of his use of sources. In chapters 4 and 5 Russell discusses Plutarch's philosophy and ethics. He accepts the view of Plutarch as "a declared and consistent Platonist" (p. 63) without acknowledging the opinions of Heinrich Dörrie and others who have recently attempted to qualify this somewhat too rigid analysis of Plutarch's philosophical orientation. The discussion of Plutarch's ethical ideas in chapter 5 is basically sound. In the course of these two chapters the reader is casually introduced to many of the works in the *Moralia*.

In chapter 6 we have a good study of the

Lives, especially with regard to their structure and Plutarch's purpose in writing them. Historians should find this discussion valuable as a guide to the use of Plutarch as a source for the individuals about whom he writes. Chapter 7 provides a detailed analysis of the *Alcibiades* as a sample biography, and in the following chapter Russell remarks upon various Roman *Lives*, in particular the *Antony*. He closes with a survey of Plutarch's influence on later antiquity and succeeding ages and takes note of the work of Jacques Amyot, Thomas North, and Philemon Holland in translating Plutarch. The brief conclusion in chapter 10 is mostly a continuation of the preceding chapter with a minimal summation at the end. The appendix contains useful catalogs of the individual works of the *Moralia* and the *Lives* in pairs, with a third section on editions and translations. The bibliography lists some of the more important works on Plutarch with very concise evaluations of several.

The author's objective, as stated in the preface, is "to explain what it is like to read Plutarch, and what I think one needs to bear in mind in order to read him with understanding." In keeping with this purpose the book abounds with passages from Plutarch's writings, some a page or more in length. Russell's presentation is at times sketchy and discursive, especially in the early chapters. In general the book is too brief and hardly does more than touch on most aspects of Plutarch and his works. It adds little to the recent work of R. H. Barrow and C. P. Jones. It is to be recommended to the nonspecialist with little or no knowledge of Plutarch as a pleasant but limited introduction to that author.

L. J. SIMMS
College of Charleston

MICHAEL GRANT. *Roman Myths*. New York: Charles Scribner's Sons. 1971. Pp. xviii, 293. \$10.00.

In the modern connotation, the study of myths is used to signify a systematic examination of the traditional narratives of a particular group of people in order that their concepts concerning religion, origin, and relations might be known. It is assumed by mythologists that the basis of mythology has a natural development

among the ordinary populace. But in his investigation of Roman legends, Grant points out that their mythology in the interest of national or family patriotism was artificially created in a piecemeal fashion over a long period of time. Thus Roman mythology must be approached with these considerations in mind. The purpose of Grant's latest work is "to offer a much more detailed treatment of the Roman mythology, treating it as an entity in its own right, and seeking to explain how it acquired its very remarkable characteristics, and how and why it became such an enormously influential factor" (p. xviii).

Accurate Roman myths deal only with stories relating to the origins of Rome itself rather than accounts of the Olympian gods. In the beginning of his study Grant considers all the Greek, Etruscan, and Latin sources of information for Roman legend. The Etruscan influence is given adequate recognition, while the Sabine element is de-emphasized. He next takes up the Trojan origin and background of Aeneas, his relationship with Dido, and the development of Roman virtue. This is followed by a review of Romulus's birth, the legendary foundation of Rome, and the affiliation with the Sabine culture. Grant rejects as pure fiction the concept that Rome was founded in the eighth century B.C.; archeology has proven that the first settlement upon the Palatine took place about 1000 B.C. Numa and the Etruscan monarchy are also given full treatment. In the conclusion the author reflects upon the question of myths surrounding the establishment of the Republic, Camillus, and the legends of later Roman history, in which era the greater Roman families effected and adjusted mythology for their own ends.

A very few Roman myths contain grains or enclosed fragments of historical truth. However, Grant's analysis produces an imaginative parahistory, which is informative because of its illumination of the Roman mentality.

The book is well written, as are all of Grant's undertakings. The work includes an excellent bibliography of recent writings, numerous plates, and detailed maps. The study will be of great value to the comparative mythologist.

STEPHEN J. SIMON
Appalachian State University

D. R. SHACKLETON BAILEY. *Cicero*. (Classical Life and Letters.) New York: Charles Scribner's Sons. 1971. Pp. xii, 290. \$10.00.

Professor Shackleton Bailey aims to value Cicero "not as statesman, moralist, and author, but as the vivid, versatile, gay, infinitely conversable being who captivated his society and has preserved so much of himself and it in his correspondence." He has accordingly quoted generously, in translation, not only from the *Letters to Atticus*, which he has edited and translated, with commentary (7 vols., 1965-70), but also from the rest of the correspondence. His translations, as always, are fluent, lucid, and exact and often catch so admirably the tone of the letters.

The method, however, has its drawbacks, as the author himself recognizes, for the space assigned to events in Cicero's life is made to depend on the extent to which they figure in the letters. That in turn depends not only on such variables as whether Atticus and Cicero were separated from one another at the time but also on the mere accident of survival. Hence Cicero's consulship, otherwise relatively well documented, receives a little over three pages, while about two-thirds of the book is devoted to the last ten years of Cicero's life. Further, though Cicero's correspondence is essential evidence for the social life of the late Republic, the Latin-less reader, for whom the book is written, needs more information than can be provided within the compass of this work.

In pursuance of his purpose the author explores Cicero's relations with others: his imbroglio with Mark Antony is well handled, and the causes and nature of his quarrel with Quintus Cicero are perceptively discussed. In the latter case, however, Professor Shackleton Bailey may rely too much on the silence of the letters. More questionably, still, he sees in Cicero's desire to excel the complement to his inner insecurity, inferring from the absence of references to Cicero's mother an early emotional lack. The suggestion is undeniably thought-provoking but less cogent if one recollects that for the ruling class of Cicero's day political ambition was a way of life, and that for the "new man," in particular, the desire to excel was a constant goad.

The author's estimate of Cicero's literary

works is distinctly cool, and he sets no great store by his actions as a politician. One is finally driven to ask, however, whether Cicero really deserves to be commemorated principally for his familial and social character, though the volume itself affords ample pleasure and amusement.

G. M. PAUL

McMaster University

JOHN BRISCOE. *A Commentary on Livy, Books XXXI-XXXIII*. New York: Oxford University Press. 1973. Pp. xviii, 370.

Books 31-33 of Livy contain a detailed narrative from 201 B.C. to 195 B.C., the beginning of one of the most exciting and most politically important eras in Roman Republican history. It begins with Rome, just after her victory over Hannibal, poised at the threshold of her adventures in the complicated stadium of Greek politics. She enters it with no realistic eastern policy at all and at the end of the game finds herself mistress of the whole eastern Mediterranean world. Livy is our primary source, along with Polybius, for this early part of the era, and generations of scholars have poured over his text and expounded its meaning from many different points of view, one of the most important being the motive or motives for the steady change in Roman policy during this second century. Some historians have seen a steady deterioration of the character of the ruling class in Rome, corrupted, we are told, by Greek luxury, aflame with new and dangerous political philosophies, conscious of Rome's manifest destiny, and convinced of its own ability to rule or dominate.

In 1965 R. M. Ogilvie published a new and detailed commentary on the first five books of Livy, and it was at once hailed in many quarters as an excellent beginning to a full and more modern commentary on Rome's greatest historian. But since then the work has not gone on, and others will have to pick up where Ogilvie left off. Briscoe's present work will fill a part of what still must be done with Livy. Preceded by forty-eight pages of introduction concerned with sources, methods of composition, language and style, speeches in the text, politics of the age, a general account of the events, and the text used by Briscoe, the

bulk of the book is confined to a very detailed and reliable commentary on every important phrase or word found in books 31–33. Briscoe has given us a very sound guide to what Livy tells us about events and politics and about the people and society of that age. Whenever the modern historian wishes further information about the meaning of what Livy narrates, he will do well to examine Briscoe's commentary with its wealth of bibliographical information. There are a few omissions, but none that should discourage use of the commentary.

It may be noted here that in the all-important matter of the Roman senate's motive for declaring war on Philip V, Briscoe believes that Maurice Holleaux was right, that the "news brought by the Rhodian and Pergamene ambassadors in the autumn of 201 had a serious effect on the senate, though they were as much affected by the news of Philip's actual aggressions as by his reported pact with Antiochus."

Adequate indexes are provided and the proofreading had been done very, very carefully, as one would expect from such a publishing house. All serious students of Roman involvement in the Greek east will have to have this book. And let us hope that Briscoe continues to work on other parts of Livy. That would permit a greater and more desirable kind of unity that would otherwise be lacking.

ROBERT K. SHERK

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MICHAEL GRANT. *The Jews in the Roman World*. New York: Charles Scribner's Sons. 1973. Pp. xv, 347. \$10.00.

MICHAEL GRANT. *Herod the Great*. New York: American Heritage Press. 1971. Pp. 272. \$12.95.

These two recent and excellent books by Michael Grant treat the same subject in different, but complementary ways. Each one is fascinating to read by itself; together they illuminate a profoundly moving and important theme in ancient history. The broad subject is the relationship between the Jews in the Near East, particularly in Palestine after it became the client-kingdom of Judaea, and the Roman government. The story is told, not as is so often done by classical historians, from the Roman vantage point, but rather from the inside out.

The focus is on the Jews and how they viewed and coped with their confrontation with Roman power.

The Jews in the Roman World opens with an introductory section that includes a chapter on Jewish traditions. In it Grant emphasizes a central point of his study—that since the Jews alone among the Greeks and Romans have had a continuous history, despite the paucity of sources for the Roman period and despite the difficulties of interpreting some of them (Josephus, for example), it is possible to reach back into Jewish history for an understanding of the inner workings of the Greco-Roman Empire. In the remaining introductory chapters Grant describes the Jewish attitudes toward Hellenization and the dissensions that emerged in Palestine during the Maccabean period. The major part of the book covers the era that began when Pompey annexed Syria in 63 B.C., and it became clear that Palestine, inevitably, would be next, and that ended when the Empire was converted to Christianity.

Roman-Jewish relations cannot be understood without first understanding the whole matrix of Greek, Oriental, and Roman rivalries that existed in the Near East. It is a complicated, though necessary, background, and in lesser hands it might simply have remained complicated. But Grant has threaded his way carefully and with great clarity through that tangle of relationships by centering on the political rivalries and providing religious and philosophical content only where they were actually relevant. For Judaea entered the Empire as a self-governing client-kingdom, and its status owed less to its distinctive religion than to its position as a buffer against the Parthian Empire. Judaea was critically important to the Romans once they became embroiled in the Near East, and few people among the Jews seem to have assessed that situation as accurately as Herod.

Grant's biography *Herod the Great* is a deeply interesting study of a man whose career was a microcosm of all the problems that confronted Judaea throughout the entire course of its relations with pagan Rome. Herod had to find a balance between the demands of his Jewish subjects that Judaea retain its Jewishness and its integrity and the demands of the Romans that he administer a loyal, pro-Roman

state. As Grant makes clear, Herod did remarkably well. His bad press has been based on the lamentable last years of his life when his family crises seem to have deranged him, on the so-called Massacre of the Innocents (for which Grant finds no reliable evidence), and on the fact that he, like Josephus, was considered a traitor by many Jews. Whatever one's sympathies are about the last charge, Herod managed to keep the Romans reasonably satisfied for most of his rule, and the Judaeans, if not satisfied, were at least economically prosperous and free from disaster.

His career illustrates some of the other complexities of Jewish-Roman relations. Herod had to maintain his throne in Judaea, but his own Jewishness did not meet the standards of most of his subjects, largely because he was a Grecophile. He spent a vast amount of energy and money to persuade the Jews of his Jewishness—one reason he lavished so much money on rebuilding the temple—at the same time that he tried to persuade the Romans that he was also indispensable to them. He wanted, or seems to have wanted, very much to be recognized by the Romans as a major figure in their world. The interesting fact that emerges from reading the biography against the background provided by *The Jews in the Roman World* is that Herod's failure in the long run emerges not as his alone, but as the failure of the Jews and Romans together to solve the problems that erupted when Jewish tradition, Hellenistic civilization, and Roman power confronted each other as they did in Judaea.

The two books are based on careful analysis of primary sources and on the latest scholarship. They are, therefore, eminently useful, and, above all, they are highly literate and a pleasure to read. Grant has maintained the high standard that he has set for himself in his previous books.

JILL N. CLASTER
New York University

PAUL MACKENDRICK. *Roman France*. New York: St. Martin's Press. 1972. Pp. 275. \$10.95.

This book, amusingly written and filled with much important data, is well worth reading for anyone with a general interest in the history and civilization of the area that is now France. In seven well-illustrated chapters—"Before the

Romans," "Caesar Slept Here," "Augustan Cities, The Fruits of Romanization," "Country Houses and Late Empire Cities," "Shrines and Statues," "Gallic Arts and Crafts," and, finally, "Roman Inspired Architecture in Modern France"—Professor MacKendrick provides us with a glimpse of his ongoing love affair with Rome and all that she influenced. In a not entirely serious vein, one hopes, MacKendrick would have us believe in the existence of a French national character going back to pre-historic times and having as its most noteworthy attributes talent in art, good taste in wine, and better in women. Most readers will be amused, few will be convinced, and the humorless *engagé* may well be *enragé*.

It may well be unfair to criticize *Roman France* from a scholarly point of view since it is manifestly a popular work. Yet all scholars, regardless of the audience for whom they are writing, have a responsibility to keep their conclusions within the limits of the evidence. Unfortunately MacKendrick often permits his imagination or his desire to tell a good story to distort his historical judgment. For example, in discussing a rich grave from near Châtillon-sur-Seine, MacKendrick concludes that its female occupant was a "queen" and that "her great wealth was no doubt derived from levying tolls upon the tin traders" from Cornwall. In another instance, while discussing the vast complex dedicated to Mercury at Puy-de-Dôme, MacKendrick concludes that "the aim of all this grandeur was no doubt to console the Arverni for the prestige they had lost when their leader Vercingetorix was defeated." The artifacts neither lie nor speak, but the archeologist or historian who would speak through them must be prudent above all else.

For teaching purposes *Roman France* can be useful as supplementary reading for survey courses in ancient civilization. Even undergraduates, however, may wonder what to think of Diocletian when they read that Julian the Apostate was "the anti-Christian Emperor" or how someone, whom MacKendrick neglects to name, spoke of Burgundy wines in 312 A.D., when the Burgundians from whom the region takes its name were settled there in 443 A.D.

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RENÉ POTIER. *Le génie militaire de Vercingétorix et le mythe Alise-Alésia*. [Clermont Ferrand: Éditions Volcans. 1973.] Pp. 367. 65 fr.

The military fame of Vercingetorix rests ultimately upon the new tactics he taught the Gauls, particularly the complex techniques of siegecraft as used above all at Alesia, where the cause of a united Gaul was finally lost to the Roman invader. Caesar's narrative is the starting point for any inquiry, but excavations begun a century ago at Alise-Sainte-Reine have convinced a majority of scholars that here we can base historical judgment upon the actual site of Vercingetorix's last stand.

In a passionately argued analysis Potier categorically rejects this identification. The trouble began, he argues, with the minor ninth-century poet Héric, who confused Alesia of the Mandubii with Alisia of the Lingones, and was then compounded by Napoleon III, who simply gave orders to dig up at Alise the earthworks of the opposing forces. Let one confront the site and excavations at Alise with the "portrait-robot" of Alisia, rigorously established on the basis of *Commentaries* 7. 66 ff., and it will be immediately clear that there exists no correspondence whatsoever—not unless one is to take liberties with the text, geography, history, logic itself. In particular, the famous ditches of Alise show discrepancies in number, proportions, and extent, being rather in the nature of drainage channels. Where then was the Gallic Alesia? Like Heinrich Schliemann, Potier scans the terrain, text in hand, sizing up the various elements of the problem—linguistic, topographical, strategic—before opting for Cornu in the Jura, a location originally proposed by A. Berthier. Everything here fits the requirements of the ancient texts, even down to the folklore of the locality, and to prove his case Potier treats the reader to a re-enactment of the campaign leading up to the siege of Alesia, the last stages of which he lets unfold about Cornu.

The author is at his best in shouldering the burden of disproof. The inconsistencies he isolates show clearly enough that "the second battle of Alesia" has still not been fought to a finish: common sense cautions against blaming Caesar wherever his information is inconveniently incongruous. Yet nagging doubts persist when one reads (p. 156) that Berthier eliminated over three hundred possible sites before

hitting upon his "ideal Alesia." For all the attractions of Cornu, irrefutable confirmation depends in the long run upon the indispensable evidence of archeological exploration. So far no dig has been possible, though Potier has scouted the area noting works still visible to the naked eye, such as remains of walls and what could be ramparts and ditches. Potier-Berthier may yet be proved right. In the meantime the prudent reader will suspend judgment.

DUNCAN FISHWICK
University of Alberta

MEDIEVAL

J. M. WALLACE-HADRILL. *Early Germanic Kingship in England and on the Continent*. The Ford Lectures delivered in the University of Oxford in Hilary Term 1970. New York: Oxford University Press. 1971. Pp. viii, 160. Cloth \$9.75, paper \$3.25.

One of the major themes of this book is that the early Germanic kings were able to convert to Christianity because certain pagan activities, especially warfare, overlapped with certain Christian duties like warfare for Christ. The author traces the progression of fifth-century barbarian overlords to ninth-century Christian monarchs with precision and elegance. We are shown how the Roman *Curia* and the Germanic kings came to terms and why the barbarians found it advantageous to be converted. A sub-theme within the larger topic is the importance of unction. The author takes a skeptical view of the necessity of anointment, putting himself in a revisionist position vis-à-vis Walter Ullmann. Another subtheme is the question of military skill as an explanation of royal power. As with unction, the author is chary of sweeping claims that kings owed their office to their prowess on the battlefield.

Since the Ford Lectures traditionally represent the distillation of a scholar's career, they are not for the beginner. The first three, at least ("Germanic Kingship and the Romans," "King Æthelberht," and "The Seventh Century"), are at times difficult to follow because of the intricacy of the argument. The references to recent German scholarship are helpful, though two omissions might be noted: apropos of Marculf (p. 49), where it is said that the

arengae of his charters amount to a mirror for princes, there is no mention of Heinrich Fichtenau's *Arenga* (1957), and apropos of royal titles (p. 111), no reference is made to Herwig Wolfram's *Intitulatio* (1967), even though Wallace-Hadrill has elsewhere shown himself familiar with these works.

In comparing kingship in England with the situation on the Continent, the author tells us that it is virtually impossible to explore this topic to its ultimate origins in "proto-Germanic history." "We cannot define it in terms of, say, the Germans of the first century B.C.; we do not know what word or words they used to describe their kings; we do not know what those kings were for" (p. 1). Nevertheless, we do have the technique of comparing lexical items in the Germanic languages for the purpose of making hypothetical reconstructions that might illumine the subject, and the author makes scant use of it. In commenting upon Tacitus's statement that the Germans had *reges*, Wallace-Hadrill offers the following: "*Rex* was an ancient and curious word. Its stem was cognate with the Sanskrit, which was also the ancestor of the Celtic word **rîgs*, from which in turn all the Germanic languages borrowed it" (p. 2). Actually it is not "curious" that Latin and Sanskrit should share the same root for the word king since both languages are descendants of Proto-Indo-European (PIE). What is unusual is that the word for king based on the PIE nominal stem *rĕg-* is found only in Celtic, Italic, and Sanskrit—fringe areas where the societies retained archaic religious bodies—and that the central languages of the Continent did not inherit it (see Emile Benveniste, *Le vocabulaire des institutions indo-européennes* [1969], 2: 9–15). Moreover, to state simply that "all the Germanic languages borrowed it" might be misunderstood without some qualification, since initially, in the case of Gothic, the inherited word *thiudans* is commonly used for king. The discussion of *rex* therefore does not convince the reader that the search for proto-Germanic kingship is really in vain, whereas the treatment of the institution of king in the historical period is most informative.

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JOSIAH COX RUSSELL. *Medieval Regions and Their Cities*. [Bloomington:] Indiana University Press. 1972. Pp. 286. \$8.95.

Professor Russell is no Johnny-come-lately to the use of quantitative methods in studying the Middle Ages. Since the appearance of his pioneering work, *British Medieval Population* (1948), his name has been associated with the application of demography to history. This latest book, an extension of his demographic interests, attempts to delineate urban regions in the century before the Black Death of 1348. The two chapters on Egypt and India are not strictly relevant to his main thesis.

Any discussion of his thesis—that the region was the "basis for European life" before 1348—must center on the methodology that underlies this conclusion. The concept or theoretical model applied to this study is that the central place or city dominates a region with which it is interrelated and thereby gives definition to the region. Other cities within the region are affected in their size and location by the central city and will tend to be distributed according to a rank-size pattern throughout the region.

One basic objection that could be raised is that a model derived from modern city patterns and verified to some extent by such criteria as circulation of newspapers or distribution of types of industries might not apply to the thirteenth century. In fact, Russell recognizes that the rank-size pattern does not hold for the earlier century: "For the medieval period it is therefore necessary to reduce the numerator [in the formula that expresses the relationship of cities]. Since the city is a two dimensional area, some form of square root reduction of the numerator seems in order and is given below." Second, it is a little disconcerting to find in some of the books cited on methodology that scholars working in the modern period are not in agreement about the model.

If Russell is right about applying the rank-size pattern, then medievalists have been given a powerful new tool that allows them to determine population figures for a city or region by extrapolation from the known figures by use of the theoretical model. The trouble with this procedure is that it sometimes leads to circular reasoning. When Russell found too many large cities too close together along the Rhine to fit

the model, he circumvented the difficulty by grouping some cities that may have functioned as a unit and, thereby, produced a reasonable pattern and region. Because the population of the region is completely unknown, he assumes that the population of the largest city (in this case Cologne) was the normal 1.5 per cent of the region, and he calculates the regional population. Later he derives an "urban index" by determining the ratio of the top ten cities to the population of the whole region and compares the density of population with northern Italy. No warning is given in the text, but on a chart printed 140 pages later the Cologne region is one where the urban index has a footnote warning that "this has little meaning. . . ." Unfortunately, this example of the treatment of figures is not an isolated one, but the saving grace is the author's complete candor which insures that the reader need not be misled by the methods being used. Although the author is clearly right in pointing out the uncertainty of many statements about the Middle Ages that are generally not questioned by scholars, the trouble with numbers and percentages is that they have an aura of precision that can be seriously misleading.

Even though the book will not lessen the skepticism among many medieval historians about the quantitative methods the author uses, he makes a valuable contribution in calling attention to the importance of regionalism in the High Middle Ages. Almost as an afterthought he poses useful questions about why and how regions developed that go beyond the correctness of each and every region he has identified. Medievalists would do well to heed his plea to examine regionalism instead of organizing all their thoughts around the theme of protonationalism. His suggestion that the region provided a barrier to the development of overpowerful monarchies is an interesting one and should not be dismissed lightly just because he overstates his case by exaggerating the democratic character of regionalism. Perhaps the best hope for quantitative methods, once proponents become less enamored with the methods themselves and opponents begin to feel less threatened by the novelty, is that new approaches will serve to break old patterns of thought and raise new questions that all will agree are worth further examination. Russell

in this book has raised some of those questions.

CHARLES R. YOUNG
Duke University

A. IA. GUREVICH. *Istoriia i saga* [The History and the Saga]. (Akademiia Nauk SSSR, Series "Iz istorii mirovoi kul'tury.") Moscow: Izdatel'stvo "Nauka." 1972. Pp. 196.

A. IA. GUREVICH. *Kategorii srednevekovoi kul'tury* [Categories of Medieval Cultures]. Moscow: Izdatel'stvo "Iskusstvo." 1972. Pp. 317.

Both of these books, written by a well-known Soviet historian who specializes in the Scandinavian Middle Ages and who teaches in Kalinin, deal generally with the same problems: the world view and mentality of the medieval man as well as his attitude toward his immediate and remote environment. The first, *The History and the Saga*, deals with the rather limited area of Norway and Iceland and discusses the people of these countries from the eleventh through the thirteenth centuries primarily on the basis of *Heimskringla*, or the saga of the kings (of Norway), which was a record of Norwegian history, particularly of her royal dynasty from the earliest times to 1177. It is traditionally attributed to Snorri Sturluson (1178-1241), an Icelandic aristocrat and historian who wrote, as well, the *Junior Edda* and who participated in the struggle between the Norwegian royal power and its feudal antagonists. Gurevich covers a wide spectrum of basic notions and attitudes of medieval Scandinavians as they are presented in *Heimskringla*—the notion of time, the function of prophetic dreams and visions, death and posthumous glory, myth and actual history, the ideal of the Konung and his relations to his countrymen, truth and fiction, and paganism among the Christianized population. Among the subjects treated, I became particularly interested in Gurevich's discussion of time in the saga. Indeed, the Scandinavians even of the thirteenth century had not yet adjusted to the Christian or any other calendar or chronological system and therefore did not place events in definite years but merely measured time in very indefinite terms of life- or reign-span. In order to show the sequel or sequence of events, the saga writer would not indicate the date or year of a given event but would recur to such

expressions as "next winter," "during that summer," "thereafter," "then," and so forth.

Exploring such notions of the writer's and of the saga's heroes, Gurevich comes to the conclusion that even in the thirteenth century, Scandinavian man underwent little effect from Christianization, despite the fact that Norway and Iceland were officially converted to Christianity two centuries before, and that he visualized the world and his relations with his surroundings primarily in terms of his original pre-Christian, pagan world view.

The scope of "The Categories of the Medieval Culture," which treats primarily the more thoroughly Christianized world of Western Europe, is wider geographically and intellectually. Gurevich deals in it with a medieval notion of the world, time, law and right, poverty and wealth, the accumulation of profit (God and Mammon), and the slow but steady crystallization of the idea of individuality. He proceeds to categorize most of the notions of medieval Western man into pre-Christian and Christian, and he tries to discover the real impact of Christianization on the people of medieval Western Europe. Both books follow a wise French principle, "populariser sans vulgariser," and are written in a clear, readily understandable style without sacrificing true scholarly methodology in exploring the subject.

SERGE A. ZENKOVSKY
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JOSEPH DAHMUS. *Seven Medieval Queens*. Garden City, N.Y.: Doubleday and Company. 1972. Pp. 333. \$7.95.

Seven Medieval Queens is an obvious, almost an inevitable, sequel to the author's *Seven Medieval Kings*. The format is the same, except that the heroes have undergone the appropriate surgery and become heroines. The book consists of "vignettes of seven outstanding women of the middle ages," as it says on the dust jacket: Theodora, Brunhilde, Theophano, Zoë, Eleanor of Aquitaine, Margaret of Denmark, and Margaret of Anjou.

When I reviewed *Seven Medieval Kings* (*AHR*, 73 [1967-68]: 114-15) I suggested that the book would be improved by the inclusion of maps, dates, an explanation of the primary sources, a prologue and/or epilogue, and a

bibliography and critical apparatus. This second work has the faults of the earlier volume and almost none of its virtues. Where the earlier volume had one eye on the scholarly audience and one on the popular, this book is completely a work of popularization. The insights are fewer and more widely separated. The writing is poor: it ranges from the simplistic to the garrulous. The lives of Theodora, Brunhilde, and Zoë are but rehashes of the relevant sections of Procopius, Gregory of Tours, and Michael Psellus, respectively, and since the primary sources are all now available in inexpensive translations, we might fairly ask for something more enterprising. Superb popular writing in a biographical mode has been done by such medievalists as E. S. Duckett, Helen Waddell, and Eileen Power. This book does not contribute to the great tradition.

I also doubt if a group of medieval queens, chosen so haphazardly, is worth our attention. Their stories are too largely the stories of their husbands' reigns. We learn about neither women, people, nor politics. There is a need for more work on medieval queenship. There is an even greater need for more studies of average women and their lives, whether done from charters, wills, dispensations, chronicles, hagiography, bourgeois literature, tax returns, court rolls, cartularies, or any other source that can be squeezed. Professor Dahmus is a solid medievalist with a useful study of Archbishop William Courtenay to his credit. He can offer us deeper and livelier stuff than sad tales of dead queens.

JOEL T. ROSENTHAL
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H. P. R. FINBERG, editor. *The Agrarian History of England and Wales*. Volume 1, part 2, A.D. 43-1042. New York: Cambridge University Press. 1972. Pp. xviii, 566. \$37.50.

The Agrarian History of England and Wales was planned in 1956 by an advisory committee of nineteen of the leading scholars in the field with R. H. Tawney, chairman. H. P. R. Finberg, professor of English local history at the University of Leicester, was named general editor. Beginning with the New Stone Age the work will extend, in eight volumes, to the present. Volume

4, which covers 1500 to 1640, Tawney's own period, was published in 1967 (*AHR*, 73 [1967-68]: 805-07)). Volume 1 is to be in two parts, issued separately. Part 1 will deal with the New Stone Age, the Bronze Age, and the pre-Roman Iron Age, and it will also include a history of livestock from the beginning to A.D. 1042. Part 2 is the book before us, and it has three sections: Roman Britain by Shimon Applebaum, professor of classical archeology, University of Tel Aviv; post-Roman Wales, by Granville R. J. Jones, reader in historical geography, University of Leeds; and Anglo-Saxon England to 1042, by Professor Finberg, who also is the editor of the book. Inasmuch as this book is priced at \$37.50 (volume 4 costs \$25.00), it would appear that the eight-volume set will be beyond the reach of most individuals. (Part 2 has already disappeared from the shelves of the Bodleian—"presumed stolen.") Applebaum's essay takes up 265 of the 526 pages in this book; the other half is shared equally by Jones and Finberg. One is puzzled by this allocation of space; the time span, roughly half a millennium, is the same in each case.

Applebaum's evidence is mainly archeological, and one feels certain that he has garnered every grain of it. He makes some reference, by actual count, to 455 sites (see the alphabetical list, pp. 268-77). All of his conclusions are tentative in some degree. The small rectangular fields of the pre-Roman period spread from the lowland to the highland zone. But consolidated land blocks divided into plow strips made their appearance in the Roman period, and some of these were cultivated by *coloni* on capitalist estates. The failure of Roman rule was due to the collapse of the superstructure followed by a slow decay of the villa system. At the grass roots level there was much more agrarian continuity between the Roman and Anglo-Saxon periods than has previously been supposed. Even so, one suspects that when Rome fell the Romano-Britons knew it.

For agrarian conditions in post-Roman Wales, that is, from the fall of Rome to the Norman Conquest, the evidence is not so much archeological as documentary. Something can be gleaned from Gildas and Nennius, and the bardic poetry of the ninth century affords sidelights. The chief source, however, is the Welsh

Law Books. Professor Jones's conclusion is that Wales was "overwhelmingly agrarian" (p. 281), and he points out that forty-two per cent of the country is below the five-hundred-foot level. But the agrarian economy was and has always been of the pastoral variety with the arable in a secondary role. The balance between plowland and pasture differed markedly during the course of the post-Roman period as palynologists have been able to show from a study of pollen stratification in peat bogs. There is an interesting exposition of the laws of cotillage, especially as they applied to bond tenants in plowlands at some distance from their village.

In "Anglo-Saxon England to 1042" Professor Finberg traverses a field where Seeböhm, Vinogradoff, and Maitland trod, but he does not follow in their footsteps. When evidence was lacking they looked backward to the unknown from the known and with the inspiration of genius sketched in missing parts of the picture. But Finberg made "a resolute rejection of hindsight"; he bases his exposition squarely on the sources. It is safe to say that no tiniest bit of documentary detail has escaped him, and he knows, too, what can be learned from archeology and field evidence. All too often, however, he finds the facts "admittedly inadequate," and one wishes he would tell us what he thinks.

If anyone still believes that free peasants with no lord over them but the king once formed the basis of Anglo-Saxon society, as even Sir Frank Stenton thought (*Anglo-Saxon England* [1943], p. 275), he should now give up. Finberg says that "for positive evidence of independent and self-governing rural communities we seek in vain" (p. 448). Lordship was basic from the start. The invaders found in Roman Britain the small rectangular fields with which they were familiar, but they also found some larger fields divided into long strips produced by a heavy plow, a more advanced agrarian procedure. By the tenth century the small field was "a thing of the past" (p. 488). Finberg's general conclusion is that at the Conquest Anglo-Saxon England had a more advanced economy and social structure than Normandy.

W. O. AULT

Boston University

M. M. POSTAN. *Essays on Medieval Agriculture and General Problems of the Medieval Economy*. New York: Cambridge University Press. 1973. Pp. 302. \$14.00.

M. M. POSTAN. *Medieval Trade and Finance*. New York: Cambridge University Press. 1973. Pp. vi, 382. \$15.00.

In no field of English medieval history have greater advances been made since World War II than in that of economic history, particularly in its agrarian and pastoral aspects. Previously the influence of William Stubbs and his writings had long given constitutional history pride of place in teaching, final examinations, and research in British universities. When that soil showed signs of exhaustion, governmental and ecclesiastical administration took up attention, but this proved less intellectually satisfying. Economic history had long been taught and examined, but emphasis had fallen upon the modern centuries from Tudor times onward. When attention was given to the medieval period the favored topics were feudalism and manorial organization as affecting agrarian husbandry, together with gilds, merchants, and urban crafts and industries. Certain broad generalizations were assumed without proof: that the quantity of free labor in England was small as a result of strict feudal control after the Conquest; that monetary and credit transactions were rare, save among merchants; and that population, wealth, and trade grew slowly but with unbroken development over the centuries between the Conquest and the Reformation.

Medieval agrarian historians were also for long mesmerized by Domesday Book. That massive, unique, and enigmatic work seemed to hold the key to every feudal, demographic, and agricultural problem of English economics of the age, and a succession of outstanding scholars—Frederic Seebohm, F. W. Maitland, Paul Vinogradoff, and later H. L. Gray and the Orwins—concentrated their attention on the circle of light it provided and discussed villis, manors, open fields, and villeins. Inevitably students and even scholars came to regard Domesday as if it were a system to which Old English practice led up and from which English agricultural, pastoral, and mercantile methods and activity developed.

In consequence, the study of the vast mass of later sources—legal, cadastral, financial, and

the rest—from which the techniques and changes of agriculture and pastoral farming, together with social and demographic changes, could be extracted was long in coming. It began with the often meritorious work of isolated researchers, such as Miss Elizabeth Levett, Miss F. M. Page, and Miss Nellie Neilson, whose work resembled exploratory borings rather than systematic exploitation, giving no scope for broadly based conclusions. The break-through, to use the current jargon, came between the wars from a group at the London School of Economics who were inspired by Eileen Power, the first holder of a chair of economic history to concentrate on the medieval period. Among her pupils and associates were Miss E. Carus Wilson and M. M. Postan, whom Miss Power married a few years before her untimely death. Shortly before their marriage Postan had moved to Peterhouse, Cambridge, and became in 1938 Clapham's successor in the chair of economic history there.

Postan's familiarity from boyhood with semi-medieval conditions in Russia, his command of Russian, in addition to the languages of Western Europe, and his competence in the fields of statistics and modern economic theory, added to what became a very wide acquaintance with documentary sources and his own powers of analysis, made of him a brilliant teacher. The influence of his writings and his personal contacts has been very great all over the English-speaking world and Western Europe, and his former pupils hold chairs all over the British Isles, with outposts in America. His earlier work was concerned with trade and finance, but at Cambridge he turned to agrarian and social history and to demography.

Postan's major contribution to historical scholarship is found in his long articles and essays in learned journals and in cooperative works rather than in books. For this reason, few even in academic circles are familiar with all his expositions that lie behind the conclusions or opinions now widely taken for granted by medievalists. The two volumes under review are therefore extremely welcome and should be precious to the younger as well as to the older generation of those concerned with English medieval economic history. Few scholars would wish or care to republish writings on controversial topics now more than forty years old.

Postan can afford to do so because he provides the essential basis and explanation of what is now the common doctrine.

The first volume, dealing with agricultural and allied subjects, contains several of his seminal expositions. That on the rise of a money economy firmly and finally dispelled the view current among students and teachers that transactions and calculations in current coin (not values only used as terms of account) were confined to fiscal circles before the gradual growth of the wage system a century or so after the Norman Conquest. The allied illusion that all agricultural and pastoral work on the manor was accomplished by customary tenants without any wage-earning labor was implicitly demolished in the brilliant article on the chronology of labor services, which was primarily concerned to show that commutation of services was relatively common in the later twelfth century, when demesnes were let out to farm, to be followed by a reaction in the thirteenth century that followed the prevailing tendency to draw back leased land into demesne in the boom period of high farming. Equally important is the introduction to an edition of the Peterborough *Cartae nativorum*, which demonstrates, in the teeth of legal theory, that villeins could acquire and transfer property by charter, a position that led to the wider conclusion that from the mid-twelfth century onward villeins without change of legal status could thrive by property transactions. They could thus ultimately reach a position of local prosperity and influence that made a rise of social and legal status possible, as one of Postan's pupils, Dr. Edmund King, has recently shown more clearly. English medieval society, though static by present-day standards, was not frozen into strata as rigid as those in some countries of Europe, both then and later. Another article shows that the then conventional view of a spectacular growth of population between 1100 and 1300, halted and reversed by the great plague of 1348-49 and its successors and thereafter gradually repaired in the century and a half that followed, must be considerably revised. The growth, slowing down considerably from the last decades of the thirteenth century, fell catastrophically with the plagues, climbed slowly up until the end of the century, and then suffered another recession till about 1470. An article on heriots and

prices on Winchester manors has an esoteric title but is full of interest, and I have never seen such direct and compelling evidence of the vulnerability of rural life to disasters of climate and disease as the graphs that show the exact correspondence of the almost vertical rise of the curves of high price in corn (that is, scarcity demand) and increased mortality around Winchester and Taunton in the wet and cold summers of 1316-17 and the plague year of 1348-49. Dr. Ian Kershaw, in a recent work, has shown that more than two hundred miles to the north, the canons of Bolton in 1316-17 lost two-thirds of their normal cereal crop, two-thirds of their sheep flock, and half their herd of cattle. Yet another article gives convincing agrarian evidence for the declining population and rising wages from the turn of the fourteenth century onward.

The second volume, with essays on trade and finance, opens with the earliest examples of Postan's expertise in discussions of credit and partnership as features of medieval mercantile and general usage from the twelfth century onward. The bulk of the volume, however, consists in three long essays: that on England and the Hanse from the *Power-Postan Studies in English Trade in the Fifteenth Century* (1933); that on the trade of medieval Europe in the North from volume 2 of the *Cambridge Economic History* (1952); and that on economic relations between Eastern and Western Europe from a cooperative volume of 1970. To these may be added a brilliant piece of condensation, a lecture on the medieval wool trade (1952). All these cover a wide field and absorb without references the results of the latest available research. They are not analytical or critical in form but the writings of an eminent historian surveying his territory, and they should be required reading for all students of economic history. Indeed, they might well provide matter for tutorial essays over a couple of terms.

These two volumes gather their contents from various sources that few scholars would have in completeness on their shelves. To those who have read only a few of Postan's articles, they will show him for what he is, a great historian capable both of extracting conclusions by thorough and careful critical examination of many original documents and also of presenting authoritative surveys of wide tracts of

history. These essays should find a place in every collection of historical works and in the bookcase of every specialist in medieval economic history.

DAVID KNOWLES
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PHILIPPE ERLANGER. *Margaret of Anjou: Queen of England*. Coral Gables, Fla.: University of Miami Press. [1971.] Pp. 251. \$7.95.

If there was a time when the university press imprint guaranteed the reader that the book at hand was respectable scholarship, that is no longer true, not after the appearance of this translation of Erlanger's biography of Margaret of Anjou (*Marguerite d'Anjou et la guerre des deux roses* [1961]). The ambitious sweep and breezy character of Erlanger's writing should have made any prospective publisher wary, although today's financial exigencies may be forcing even university presses to print anything that promises a profit. Most deplorable is the intellectual defilement suffered by the unsuspecting reader who swallows this pseudo history on the assumption that the university press label constitutes the historical profession's imprimatur. In this instance that danger is doubly real since the book promises to right all the slanders Margaret has suffered at the hands of earlier writers.

Actually Erlanger suggests several slanders of his own. He assigns Margaret more lovers than does even Shakespeare, whereas in all probability she had none. The author has her striking the bloodied face of the slain Richard of York, then on the chance the morbid among his readers would prefer something more savage, Erlanger cites Shakespeare's lines that have her stabbing Richard to death. The remorse that Henry VI felt over the bloody manner in which his grandfather secured the throne was what drove him to prayer and asceticism. Humphrey of Gloucester, although "outraging any woman who took his fancy" (p. 58), was immensely popular with Londoners. Englishmen of the fifteenth century continued to treasure the liberties assured them by the Provisions of Oxford. Still, these English were scarcely civilized. "In England a meal was composed of nothing but highly spiced meats, and it was customary for the guests to get drunk thereafter. . . . To unleash a pack of hounds and hunt

some poor wretch over the countryside was taken for an amusing pastime to which one invited one's neighbours" (p. 84). When Cardinal Beaufort died—he had once planted a would-be assassin under Henry V's bed—"no man who beheld the dying rictus could have doubted that he had gone straight to hell and damnation" (p. 103). Margaret's son Edward did not die on the battlefield at Tewkesbury. He was captured, abused by Edward (IV), and then hacked to pieces by Edward's retainers.

These and other historical monstrosities suggest the plea: if society ever decides it proper to burn books, let this be among the very first to go.

JOSEPH DAHMUS
Pennsylvania State University

G. W. S. BARROW, editor, with the collaboration of W. W. SCOTT. *The Acts of William I, King of Scots, 1165–1214*. (Regesta Regum Scottorum, volume 2.) Edinburgh: Edinburgh University Press; distrib. by Aldine Publishing Company, Chicago. 1971. Pp. xvi, 549. \$17.50.

Now that printing costs threaten to make the publication of texts *in extenso* prohibitively expensive, it is a peculiar pleasure to come across a substantial and elegant volume of texts at a price that is comparatively modest, thanks to a generous grant from the Carnegie Trust for the Universities of Scotland and financial support from the British Academy.

Five hundred and twenty-seven *acta* are transcribed in full, each preceded by a summary of the contents and followed by an elaborate *aparatus criticus*. A further seventy-three *acta*, which have not survived in full, are calendared from partial transcripts. Over a hundred pages of introduction are devoted to "The Life and Reign of William I," "Scottish Royal Government, 1165–1214," a diplomatic analysis of the *acta*, and a skeleton royal itinerary. There are also four pages of additions and corrections to volume 1. There are separate indexes for persons and places and subjects.

The long reign of King William the Lion saw the consolidation of the Scottish monarchy, and it removed the uncertainties as to the survival of the dynasty, which had plagued the reign of his predecessor and brother, Malcolm the Maiden. "Paternalistic and autocratic, conservative in ecclesiastical affairs," as Professor Barrow writes, "William upheld for nearly half

a century the proposition that there was such a thing as the kingdom of Scotland and that he was in charge of it."

The publication of this second volume of the *Regesta Regum Scottorum* is by any reckoning a notable event in Scottish historiography. The political and ecclesiastical aspects of the reign are well enough known, but much of the fundamental work of consolidating the monarchy lay in the encouragement of royal burghs, the fashioning of a workable system of royal justice, and the establishment on the royal demesne of a close-knit ruling class holding by military tenure. In illuminating these lesser-known aspects, this edition of William's *acta* places our understanding of his achievements on a footing of impeccable scholarship.

It is not only Scottish historians, however, who will find this collection of texts invaluable. Its numerous illustrations of the process of Norman settlement offer many interesting parallels, and equally interesting contrasts, to Norman settlement in England, Wales, and Ireland. On the estates of the king of Scots the displacement of thanes by feudal lords was an inexorable but gradual process. In document 281, for example, we see them side by side. In a mandate to his faithful men of Moray, King William commands that if a serf refuses to pay tithe, the thane in whose jurisdiction he lives, "or his lord if he have a lord," shall distrain upon him to do so.

A minor criticism of this edition is the frequent use of Scottishisms without explanation. *Merks*, *toun*, and *brieve* may be readily intelligible, but considerably more difficulty may be caused, for example, by *teind* (tithe), *neyf* (serf), *stank* (millpond), *poind* (a pledge). The use of Scottish technical terms is, of course, justifiable, but a concession might have been made to non-Scottish readers by reprinting in this volume the glossary that appears in the first volume of the *Regesta*.

A second criticism is the placement of the extensive notes to the introduction at the end of each section. Since they are more than references and frequently carry a major part of the argument, they deserve a place at the foot of the page. It is a blemish on an otherwise immaculate production.

W. L. WARREN

Queen's University of Belfast

KATHLEEN HUGHES. *Early Christian Ireland: Introduction to the Sources*. (The Sources of History: Studies in the Uses of Historical Evidence.) Ithaca: Cornell University Press. 1972. Pp. 320. Cloth \$11.50, paper \$4.95.

If it is agreed that documentary materials are critical for historical study, the student of Irish history from the sixth to the twelfth century is in a weak position. When, in an earlier volume in this series, G. R. Elton reviewed the sources for the history of England, 1200-1640, he allotted seven out of eight chapters to documentary sources. Miss Hughes, by contrast, is forced to offer a different proportion: only five of nine chapters can be called hard-core historical material. Clearly, then, the historian of early Christian Ireland must rely on the data and implications of ancillary sciences.

Historians can learn much from their sister disciplines, and when written sources are scarce or inadequate, they must. Miss Hughes has intelligently and informatively surveyed current scholarship in the fields of archeology, literature, hagiography, and the plastic arts as it bears upon her prime target. She is refreshingly clear and exact as to the nature of the information available and its utility for the professional historian.

Chapters on ecclesiastical learning and historical writing in the eleventh and twelfth centuries are useful surveys that are augmented by Miss Hughes's attempts to relate the nature of intellectual activity to the social conditions of the time. For instance, she offers cogent suggestions as to why the material is written in Irish rather than in Latin when the reverse had been true in an earlier time.

In dealing with secular laws, ecclesiastical legislation, and the annals, Miss Hughes shows what historians have available now and what remains for them to do. Most of the sources, in her opinion, need either to be edited and published or mined anew. The sort of task that awaits is amply illustrated in her chapter on the annals, a chapter twice as long as any other. Step by step she reviews the work of scholars who have established manuscript traditions to show how diverse annals were united to form the tenth-century Chronicle of Ireland. To many it may seem that this chapter is overly detailed, but by way of compensation it may be argued that the exposition of methodological

problems is as valuable as a catalog of sources, especially when that catalog is as sparse as this one.

Irish history in this period is little known, and so this guide to the sources will prove useful. Beyond this, however, the book is a mine of information on early Irish life, traditions, and institutions. Written with clarity and restraint, this volume sustains the quality of the series in which it is numbered.

COBURN V. GRAVES

Kent State University

SUMNER MCK. CROSBY. *The Apostle Bas-Relief at Saint-Denis*. (Yale Publications in the History of Art, 21.) New Haven: Yale University Press. 1972. Pp. xvi, 116, 86 plates. \$10.00.

In 1947 the author, continuing work begun in 1939 at the church of Saint-Denis outside Paris, uncovered "a modest plaster sarcophagus sealed with a stone cover" (p. 9). The present monograph gives an account of the discovery, a description of the bas-relief that was used, upside down, as a coffin lid, and an analysis of the style and original function of the carving. The grave is dated to the late thirteenth century, the relief on stylistic grounds to the 1140s, the period of Suger's renovation of the church. From the edge carving Crosby interprets his find as part of the altar Suger built over the *confessio* of the saint. Stylistically the carving seems to have more in common with metal than with stone work. Crosby conjectures that the relief was abandoned as the result of the unexpected volume of gold contributed to Suger's project.

In its main lines the argument is neat and attractive, though weak in structure and use of sources. Compare the tentative suggestion that "[various stylistic elements] add supporting evidence to a Lorraine provenance for the artist" (p. 56) with the unequivocal statement on the "artist . . . of Mosan origin" (p. 65). In writing of paleography, by which he means epigraphy, Crosby explains the crude inscriptions by calling the craftsman "[u]nlettered, as were most of his fellow artists" (p. 56). How, then, may one account for the truly calligraphic inscriptions on the contemporary ivory casket from Saint-Denis (fig. 67) and the tympanum from Saint-Bénigne (fig. 82), to which the author refers in another

context? The central difficulty in the vogue word "program"—patron or artisan?—is not recognized. In the representation of twelve apostles it involves the elimination of one from the total of thirteen, counting Matthias and Paul but omitting Judas Iscariot. In the bas-relief the first on the left is unnamed and can be either Thaddaeus or James the Less, not James alone (p. 11). In the ivory mentioned, Paul is again included but Matthias is omitted. Crosby is probably right in saying that except for Peter and Paul in the middle, the apostles are arranged without reference to seniority of call, family relation, or liturgical commemoration. On the other hand, Crosby's invalid reference to the liturgical year (p. 53 n.61) should serve as a caution against self-help instead of expertise. By the same token the unrecognized echo of Gen. 28:17 (p. 80 n.5)—"terribilem esse . . . locum," used as an antiphon for the dedication of a church—would be better rendered as "awesome" rather than "horrible." Similarly the equation of apostles with columns (compare p. 88 n.32) is scriptural rather than exegetical (Gal. 2:9).

Medieval personal names will always be a problem for a copy editor, but Abbot Pierre de Saine Fontaine (p. 61), Jocelinus, bishop of Soissons, and Rupertus of Deutz (both on p. 92 n.61), together with Bernard de Clairvaux (p. 94 n.10), are an unlvely anthology asking for uniform treatment.

J. D. BRADY

American Numismatic Society

J. N. HILLGARTH. *Ramon Lull and Lullism in Fourteenth-Century France*. (Oxford-Warburg Studies.) New York: Oxford University Press. 1971. Pp. xxvii, 504, 16 plates. \$34.00.

As the author remarks in his preface, it was an afterthought to begin his book with two long chapters on the life and world of Ramón Lull. Otherwise his title could mislead one, for while the opening chapters probably constitute the best-informed and most up-to-date survey of the Catalan mystic and his program, the centerpiece of the study, occupying an equally large section, concerns the Parisian doctor of medicine, Thomas Le Myésier, whose *Electorium magnum*, based as it was on Lull's Art, receives here its first scholarly treatment.

As a biographer Professor Hillgarth is a partisan of his subject: he cannot be faulted in his knowledge and understanding of Lull, but his attempts to change the traditional view of Lull as an idealist and dreamer, a man after his time and out of touch with reality, will probably find hard going. The old view needs some amendment, but not quite as much as he provides us with. As a realist Lull might have taken his degrees at Paris instead of trying to gain audience there late in life after he had been tutored and largely self-educated on the frontiers of Christianity in Majorca.

It was fortunate for Lull that he acquired a stout and well-placed disciple in Thomas Le Myésier, whose work is the "Lullism" of the book's title. Having studied theology at the Sorbonne, he knew the theologians, although his vocation was medicine and he served as personal physician to Mahaut d'Artois for most of her adult life. Believing in Lull but realizing that the obscurity of his language and the complexity of his Art repelled most students, Le Myésier resolved to make Lull easier to understand and more accessible to the university world. He anthologized Lull's works in a series of four compilations whose sizes ranged in descending order, the smaller anthology distilling and drawing from the larger. Of these, the largest, called the *Electorium*, and the third, known as the *Breviculum*, have survived, and Hillgarth gives a thorough analysis of the former. Le Myésier intended through the *Electorium* to convince the Parisian masters of the supreme value of Lull's Art. Superior to Aristotle's logic on which it was based, the Art was a system of education that enabled man to get at the unity of all knowledge and divine truth, and so to convert the infidel by demonstrating the proof of the Christian faith. Unsuccessful in the fourteenth century, Le Myésier's work may have influenced Pier Leoni in the fifteenth. But Lullism, condemned by Jean Gerson and the university, found more important followers in later centuries, and this is the subject of a final section.

Hillgarth's work is a remarkable accomplishment of scholarship. Two hundred pages of appendixes include a folio analysis of the *Electorium*, an edition of Le Myésier's "Introduction to Lull's Art," and a valuable study of the twelve miniatures of the Karlsruhe *Bre-*

viculum, which the author believes was presented to Jeanne of Burgundy-Artois, Philip V's queen and the daughter of Mahaut d'Artois. Plates of the miniatures complete the book.

FRANKLIN J. PEGUES
Ohio State University

SALVATORE FODALE. *Comes et Legatus Siciliae: Sul privilegio di Urbano II e la pretesa Apostolica Legazia dei Normanni di Sicilia*. (Università di Palermo, Istituto di Storia Medioevale. Studi, number 2.) Palermo: U. Manfredi Editore. [1970.] Pp. 148. L. 3,500.

The special relationship between the kings of Sicily and the papacy and their exceptional jurisdiction over the church within their kingdom engendered a controversy that lasted well into the eighteenth century, for an ecclesiastical-diplomatic problem that originally involved the Norman rulers eventually became an issue for the Spanish monarchy. Moreover, misunderstandings about the relevant historical facts have persisted even to the present. The root of the controversy was the question of the authenticity and/or interpretation of the bull, *Quia propter prudentiam tuam*, issued by Urban II in July 1198.

This extraordinarily confused, yet highly significant problem is analyzed in this thoroughly documented monograph by Salvatore Fodale. He explains how the bull was included in a collection known as the *Capibrevi* (ca. 1508) whose compiler, Giovan Luca Barberi, was delegated by Ferdinand the Catholic to document all the relevant royal prerogatives. No source was given, but Fodale believes that the text came from a passage in a fourteenth-century or later edition of Geoffrey Malaterra's chronicle. Barberi interpreted the bull and other documents, including one dated 1477 that Fodale discovered in the Palermo archives, as the concession of an ecclesiastical authority amounting to a permanent legateship to Count Roger and his heirs. Fodale also maintains that the novelty of Barberi's conclusions has not been sufficiently emphasized and that, in general, the Sicilian rulers did not claim legatine powers before the edition of the *Capibrevi*. Moreover, before Caesar Baronius no other authority had cited the bull and no original copy had been found.

Curial opposition to the legatine claims and

challenge to the authenticity of the bull began in earnest after the Council of Trent, notably in the work of Baronius. He was in turn answered by champions of the Spanish monarchy, and the controversy continued until the end of Spanish-Bourbon rule in Sicily in 1860.

At this point an important work by F. J. Sentis, *Die "Monarchia Sicula"* (1869), opened what Fodale calls the "modern historiography of the Legation." Here for the first time a letter of Paschal II (dated 1117), which referred to the concession of his predecessor, was cited. This, according to Fodale, seemed to lay aside any doubt as to the authenticity of Urban II's bull, but a careful examination of its wording does not substantiate Malaterra's statement about the granting of a legation. Rather, it appears that Urban, probably on his own initiative and in order to normalize relations with the Sicilian ruler, agreed not to send a legate without the count's approval and to rely on him to implement any directives coming from Rome. Thus the pope accepted a limitation on papal intervention in Sicily, a limitation, as Fodale notes, that was not exceptional and not unlike that demanded by Norman rulers in England. Moreover, except for an appeal by Roger II that elicited the letter of Paschal II, Urban's bull was not cited, nor was it mentioned by other contemporary chroniclers. In fact it became an issue only in the sixteenth century.

Fodale has given a convincing presentation of his conclusions. But since it scarcely seems likely that a controversy that has lasted from the Middle Ages to the present will die down immediately, these conclusions may well provoke further discussion. In any event, this monograph, based as it is on careful research, should place the so-called Norman legation in its proper perspective. Fodale has made a significant contribution to the understanding not only of Norman-papal diplomacy, but of the relations between the curia and the later Spanish monarchy, as well as the involved historiographical controversy that followed.

MARSHALL W. BALDWIN
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ROBERT LAWRENCE NICHOLSON. *Joscelyn III and the Fall of the Crusader States, 1134-1199*. Leiden: E. J. Brill. 1973. Pp. viii, 232.

Joscelyn III de Courtenay, seneschal of the Latin kingdom of Jerusalem and titular count of Edessa, was an important and at times powerful figure in the twelfth-century crusading states. Relatively little hard evidence about his career has survived, so that writing a biography of him in a modern sense is really not possible. What Professor Nicholson has essentially tried to do is to bring the scanty evidence together and to write a history of the Latin settlements in the East during Joscelyn's lifetime—roughly from the mid-1130s to the end of the century. This is a natural outgrowth of the author's earlier studies of Tancred and Joscelyn I.

The aim is admirable. The result is not wholly what one might have wished. The book needed the attention of a rigorous editor, but it failed to get it. This is a pity, for the book is blemished by stylistic lapses that could easily have been corrected. More distressing still are the shortcomings in research. Nicholson has combed the older editions of the sources and the older studies of the crusading states with reasonable diligence. He has not taken account of recent work dealing with the period he covers. Thus, for example, he writes at some length on the Battle of Hattin, in which Joscelyn took part. But the author fails to take account of the very important treatments of that battle by C. R. Smail (*Crusading Warfare* [1956]), Joshua Prawer ("La Bataille de Hattin," *Israel Exploration Journal*, 14 [1964]: 160-79), and Peter Herde ("Die Kämpfe bei den Hörnern von Hittin und der Untergang des Kreuzritterheeres (3. und 4. Juli 1187): Eine historisch-topographische Untersuchung," *Römische Quartalschrift*, 61 [1966]: 1-50), which present quite different interpretations of the course of the battle and the leadership of the armies, based in part on fresh examination of the terrain itself. Recent editions of sources also have escaped the author's notice, for example, the new edition of the original version of the *Itinerarium peregrinorum* by Hans Eberhard Mayer (1962). Likewise, Nicholson seems not to have consulted much of the abundant recent literature on the society, economy, and institutions of the Latin states. There is not even a reference to the massive two-volume history of the Latin kingdom by Prawer (*Histoire du royaume latin de Jérusalem* [1969-

70]), to name only the most obvious of the missing references.

JAMES A. BRUNDAGE
University of Wisconsin—
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MODERN EUROPE

ERNLE BRADFORD. *Christopher Columbus*. (Studio Book.) New York: Viking Press. 1973. Pp. 288. \$16.95.

This book is a popular retelling of the life of Christopher Columbus that throws no new light on the Admiral of the Ocean Sea, but that is partially redeemed by the quality of its illustrations. Sumptuously produced in Italy Ernle Bradford's *Christopher Columbus* is filled with beautiful color reproductions of early paintings and maps and color photographs of some of the West Indian beaches discovered by Columbus, as well as with reproductions of woodcuts from the literature of the period of discoveries. Unfortunately the illustrations, as well as the text, occasionally mislead. Thus the Genoese world map of 1457 is captioned "Paolo Toscanelli's world map, c. 1457" and related to the map sent by the Italian savant to the king of Portugal along with a letter describing the possibility of sailing across the western ocean to Asia. Bradford's caption flies in the face of the prevailing scholarly opinion that denies the attribution to Toscanelli and that particularly rejects the view that this map was the map sent to the king. On the subject of Columbus's navigational ability Bradford shows himself more skeptical than most scholars. Columbus's choice of a northern route home on the first voyage, for example, is attributed to miscalculation rather than to skill. Bradford's criticism of Columbus's administration of the Indies in the early years, while following the prevailing scholarly opinion, shows little sensitivity to the Admiral's still valid complaint that he was being judged as if he had been a governor sent to Sicily "or to a city or town under regular government, where the laws have long been laid down and people are used to observing them." Whatever the scholarly limitations of the text (which is well written throughout) the book delights by its illustrations.

WILCOMB E. WASHBURN
Smithsonian Institution

J. H. HEXTER. *The Vision of Politics on the Eve of the Reformation: More, Machiavelli, and Seyssel*. New York: Basic Books. 1973. Pp. xii, 243. \$8.50.

Professor J. H. Hexter enjoys on both sides of the Atlantic a high reputation for characteristic and distinguished essays on the thought and practice of early modern society, and from time to time he gathers a group of related essays into a book. The volume under review has as its centerpiece a long study of Thomas More and his *Utopia* set within the framework of Europe in the early sixteenth century. It was originally published as the historical introduction to the Yale edition of *The Complete Works of St. Thomas More*, but it is here somewhat enlarged, since the section on "Utopia and Geneva" (originally excised from it and published elsewhere) is now restored. It also includes two appendixes, one of which suggests the time order in which *Utopia* was written (important in itself but inevitably speculative), while the other deals with the chronology of More's stay in Antwerp. The remaining essays in the book comprise an introduction that considers the basic assumptions of early sixteenth-century political theory; a close analysis of *Il Principe* in its use of the words "*lo stato*," meaning not, Hexter argues, the modern political and territorial entity, but rather "an instrument of exploitation, the mechanism the prince uses to get what he wants"; an examination of the language of *Il Principe* and *Utopia* in search of the "fabric of imperatives," that is, the governing principles of their policies; and finally, a reconsideration of the philosophies of these two works and a discussion of Seyssel's *Monarchie de France*, representative of a conservative approach to the role of kingship in the state. These essays are not of the same high order as the central study, and there is also a certain amount of repetition, but they are stimulating and provocative, and they will encourage reflection and debate.

But the central essay on *Utopia* is masterly in its study of the evolution of More's philosophy. Hexter's dissection of Chambers's version of More, as the great medieval figure standing out against the advancing modernity of the state, leaves little of it standing; nor, he shows, is More the Christian humanist and Erasmian

tout simple, though he owes a great deal to both. Rather, More went back to the roots of Christianity and to the Bible, but here Hexter stresses a profound yet subtle difference between More and his like-minded contemporaries. *Utopia* is not one more account of the education of a Christian prince; it is the rejection of a compromise between Christianity and the unequal and unjust society of early modern Europe. In that sense it was, and remains, a revolutionary work.

It is impossible to do justice to Hexter's patient, lively and refreshing account that raises More above both the hagiographers who overlaud him and the disparagers who see him as a threat to the emerging sovereign state. I continue to hope, as I did when I first read it, that Hexter's study of *Utopia* will also appear as a paperback. Then indeed it will reach, as it deserves to, the wide audience of students of history, literature, and political theory of the age.

JOEL HURSTFIELD
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FRIEDRICH HEER. *Europe, Mother of Revolutions*. Translated from the German by CHARLES KESSLER and JENNETTA ADCOCK. New York: Praeger Publishers. 1972. Pp. 368. \$12.50.

Compared with Heer's other writings, especially *The Intellectual History of Europe* and *The Medieval World*, this book, which covers major European intellectual developments from the French Revolution to World War II, is disappointing. Science, technology, and their impact on European thought are given short shrift, while theological issues take up fully a third of the book. Freud, Weber, the existentialists, and a host of others who revolutionized modern European thought are also passed over. Except for one chapter on Russia, the book deals almost exclusively with Western European developments: romanticism, Marxism, the German national movement, the Saint-Simonsians, and late nineteenth-century utopian thought. And what Heer has to say about them, although often suggestive, is largely unoriginal. As for the title, who ever doubted that modern Europe fostered and spread revolution? The more important question, which this book raises but does not confront, is

why, in the face of fierce and sustained resistance, the forces of revolution won out in Europe, and whether this development was a unique phenomenon or a process possible anywhere in the world under the right circumstances, with or without Europeanization. Heer is sparkling on premodern Europe, less so on the modern period.

Still, there is much to commend this book. The longest and best chapter by far, "Problems of Catholicism," does on a small scale for modern European Catholic thought what H. S. Hughes did for social theory and Marcuse for radical thought. Heer's treatment of the politicization of French Catholicism, Catholic attitudes toward the Dreyfus affair, modernism and reformism within the Church, Russian attitudes toward Rome, and many leading recent Catholic thinkers succeeds in structuring a surging, complex world of thought, compared with which much of the rest of the book seems fragmented and aimless. The last chapter, "Russia in Europe," a searching examination of ambivalent Russian attitudes toward the West from Belinsky and Gogol to Trotsky and Stalin, is also outstanding. Both chapters cover ground on which Heer is at his best: religious thought and controversy with their intellectual and existential ramifications.

The book as a whole cannot claim to be an original contribution to our thinking about recent European thought and culture and their current significance. But it is the work of a distinguished intellectual historian and prominent figure in contemporary Austrian cultural life, who almost always has something worthwhile to say.

ROBERT ANCHOR
University of Southern California

HILDA I. LEE. *Malta 1813-1914: A Study in Constitutional and Strategic Development*. Valletta: Progress Press. N.d. Pp. 292.

The French expedition to Egypt in 1798 served to alert Britain to the importance of the Mediterranean for her imperial purposes. Even though in 1801 Pitt asserted that "the Mediterranean is but a secondary consideration" he had by 1803 come to the conclusion that "our possession of Malta was essential." Gibraltar, Malta, the Ionian Protectorate (even though

relinquished in 1863), and Cyprus (after the opening of the Suez Canal) are the stages through which the Mediterranean came to be the lifeline of the British Empire during the nineteenth century when Britain was the chief naval power in that sea. The position of Malta was central in all senses of the word.

Until the last decade of the period France was the obvious threat, especially in possible conjunction with Russia. But the decline of French power, replaced by the rise of German might, eventually placed the two chief imperial powers in the same defensive position. This found expression first in the 1904 Entente Cordiale, then in the 1912 naval arrangements.

Malta was not a Crown colony, and considerations of defense had first priority in the rule of the island. But the people of Malta are sufficiently numerous to make the problem of their governance different from that of Gibraltar. Thus the issue arose of the degree of control that could be granted them in the management of their own affairs. Given also the overwhelmingly Catholic allegiance of the Maltese people and their common language (Italian among the educated), religion and language were often the concrete issues around which Maltese politics centered.

Constitutional arrangements, the make up and powers of the Council of Government, and the numbers and powers of its elected members, were the subjects of unending discussion between the British government and those elements in Malta desirous of extending the range of Maltese control. The detailed discussion of these problems, in the context of overriding strategic requirements, is the substance of this book. Despite the annoying typography the task is done in thorough fashion, furnishing in the small an interesting and useful case study of the operation of that impressive creation, the British Empire.

RENÉ ALBRECHT-CARRIÉ
Columbia University

IRBY C. NICHOLS, JR. *The European Pentarchy and the Congress of Verona, 1822*. The Hague: Martinus Nijhoff. 1971. Pp. xiii, 363. 49.50 gls.

This study is more than just a factual account of the Congress of Verona; it is a diplomatic historian's dream come true—the unrestricted

freedom to elaborate one's treatment of the subject. The author in this case was permitted to have two background chapters, two more on the Vienna preliminaries, six chapters on the Verona deliberations, four chapters on historiography and interpretations, a concluding chapter, and, in eleven of these chapters, subsections devoted to still more retrospections. No subject could have been more thoroughly narrated and evaluated. With convincing substantiation the author tends to justify the attitudes and policies of Wellington, Villèle, and Metternich, but to criticize those of Alexander I, Montmorency, and Chateaubriand. His conclusions seem sound when he asserts (p. 135) that France was not commissioned by the Alliance to attack Spain, that by the withdrawal of Britain the Alliance was broken (p. 324), and that out of the ruins of the congress system arose the concert system of conferences (p. 325). In addition to the main question regarding Spain the book deals at length with those of the Near East, Italy, Latin America, and the slave trade.

On one point, however, the author seems to be in error. He asserts (p. 59) that "recent historians have ignored completely the Vienna conference," but he overlooks Bertier de Sauvigny's study of a year earlier, *Metternich et la France*, which devotes twenty pages to it as compared to his own twenty-two. Also Schroeder's study of 1959, *Metternich's Diplomacy at its Zenith*, which the author does utilize, devotes five pages to the so-called "ignored" conference. He criticizes a few other historians (p. 59) for calling Vienna a "preliminary" conference, but nothing in this book gives the impression that it was more than preliminary.

As to sources it is disappointing that, while dealing with Europe's "Pentarchy," the author used only the British and French archives. Although he did consult a considerable amount of Russian printed documents he did not seem to have used any Prussian printed or archival sources and neglected entirely the rich Austrian archives, except by indirect use through Schroeder. These omissions and the failure to consult the accessible Piedmontese and Spanish archives or Becker's *Relaciones exteriores de España* makes this study less than definitive. Yet, at the same time, it must be conceded that this work deserves to be recognized as the most

exhaustive study of the Congress of Verona and probably the last one that will be attempted.

LYNN M. CASE

University of Pennsylvania

ULRICH KRÖLL. *Die internationale Buren-Agitation, 1899-1902: Haltung der Öffentlichkeit und Agitation zugunsten der Buren in Deutschland, Frankreich und den Niederlanden während des Burenkrieges.* (Dialog: Schriftenreihe für Publizistik- und Kommunikationswissenschaft, 7.) Münster: Verlag Regensburg. 1973. Pp. 478. DM 48.

The South African War, 1899-1902, marked a watershed in the history of British imperialism. The war started as a colonial conflict fought by British professional soldiers against an enemy thought to be hardly superior to warlike Indian mountaineers. It expanded into a South African civil war that pitted Briton against Boer, and sometimes even Boer against Boer. It culminated in a huge logistic enterprise involving the dispatch from Britain and her Dominions of nearly 400,000 men to fight on the other side of the globe, a feat then unprecedented in the annals of war. The Boer War finally turned into a classical guerrilla struggle that engendered bitter political dissensions in Britain herself. British wartime chauvinism soon evaporated. Wartime disillusionment seeped into anti-imperialist critiques such as Hobson's *Imperialism: A Study*. This influential book was in part a product of the author's South African experience and was praised by Lenin.

The Boer War also aroused widespread agitation on the Continent, where popular sympathies were almost entirely with the Boers. The author's study, originally a doctoral dissertation, provides a detailed account of the various pro-Boer movements in Germany, France, and the Netherlands. His is a thorough, well-documented monograph, based on a wealth of published and unpublished sources in South Africa, France, Holland, and East and West Germany. Kröll stresses the extraordinary political diversity that distinguished the pro-Afrikaner camp. Boer sympathizers included pacifists anxious to put an end to bloodshed on the veld; pan-Germans who gloried in the military might of the fatherland; humanitarians

hopeful of establishing a new international rule of law; and French chauvinists determined to avenge Fashoda. For a short time the Boers became the most popular people in Europe, lionized in the same fashion that Polish and Hungarian insurrectionaries had been lionized during the earlier part of the nineteenth century. But the pro-Boers were usually quite ignorant of South Africa's wider social and racial problems. In this respect they resembled Polonophiles and Magyarophiles of an earlier period, men and women unacquainted with the realities of Eastern Europe, where the insurgent nationalism admired abroad often represented no more than the interests of locally dominant socioethnic minorities.

While the pro-Boers differed vastly among themselves they had one feature in common: a marked proclivity to project their own domestic preoccupations onto an African screen. Admirers of the German folk soul spoke of the Transvaalers as if the Boers somehow formed part of a greater German people, an assumption unrelated to the facts of history. Socialists accused the British of shedding blood for the sake of gold; yet the war had nothing to do with the proprietary rights in the South African mining industry. Romantics enamored with the mystique of blood and soil created for themselves an idealized image of virtuous Boer patriarchs, untainted by the love of gain, but these were mythical figures who existed nowhere except in newspaper columns and novelettes. Moreover the pro-Boer sentiments were, in the long run, apt to run counter to the particular national interests of their foreign spokesmen. Once the war was over international agitation died down and the romance went out of Afrikanerdom. In the final analysis the pro-Boer campaign in Europe was an essay in futility.

L. H. GANN

Stanford University

FRANCIS ROUTH. *Early English Organ Music from the Middle Ages to 1837.* New York: Barnes and Noble. 1973. Pp. x, 305. \$17.50.

As an impressive work of reference and a perceptive commentary by an experienced performer on a great corpus of music, this book places in its debt students of British culture.

Mr. Routh, an organist and musical editor, has sought both to locate the manuscript sources and to provide a critical listing of editions for the hundreds of organ compositions from around 1400 down through the death of Samuel Wesley in 1837. Routh, as well as having compiled a catalog, compels our admiration for his delineating the historical position and illuminating the artistry of many a composer, as for example the case of the transitional eighteenth-century figure John Keeble.

Routh upon occasion also succeeds in tracing a series of interactions between music and general history that result in the art's developing a resiliency enabling it to survive more directly cataclysmic general events. The best example is his analyzing, first, the development after the Reformation of the "voluntary" as nonliturgical music, based on free inspiration, not plain chant, for performance in places of worship and, second, the rise, also after the Reformation, of domestic music making, which of necessity put a premium on keyboard skill, both upon the virginal and the chamber organ (the latter of which was recognized by the mid-seventeenth century as the instrument better serving as a foil in concerted works to the viols so prominent under the early Stuarts). The conjunction of these two lines of development, Routh argues, produced a musical school of such strength that Puritan pressure for removal of organs from places of worship could not eradicate it.

In Routh's opinion such resiliency is simply a manifestation at one point in time of the strength of "an unfolding, immensely variable, yet continuous, tradition" of English organ music, which forms the central thread of his book. Now it is about Routh's articulation of this "tradition" that I must entertain grave reservations. It seemingly embraces both the most brilliant and the most indifferently gifted of composers. The reader is at a loss to discern any consistent line of reasoning either as to the sources of the tradition's strength or the causes for its demise—especially so for the latter, as the tradition is said to have been both consummated and terminated in the career of Samuel Wesley. How odd that Wesley should have failed to propagate the "fresh techniques and styles of organ composition" with which Routh credits him! Did the fault lie in the

deterioration of the traditional matrices of musical education? Routh is concerned about the absence in the eighteenth century of the best organ composers from the Chapel Royal. Or could the fault have lain in the social character of music by Wesley's time, or in the activities of early nineteenth-century musical organizations? Routh suggests no answers along these lines, and his misunderstanding of the nature of the entertainments at the Argyll Rooms and of the foundation of the Philharmonic Society indicates the work that remains to be done.

DAVID WARREN HADLEY
Wake Forest University

STEPHEN J. GREENBLATT. *Sir Walter Raleigh: The Renaissance Man and His Roles*. (Yale Studies in English, 183.) New Haven: Yale University Press. 1973. Pp. xii, 209. \$7.95.

A prime merit of this book is that it is short. Its thesis glares: there is a tense, tight relationship between art and life, imagination and action in Raleigh. Since the words "public" and "private" did not have the same meaning in Raleigh's day that they have since acquired, Raleigh's private verses were in effect public virtues. Raleigh was above all an actor playing parts for an audience, in particular the queen. If he studied most to please himself, to play the role of the man he wished himself to be, his failure lay not in lack of skill, but, ironically, in his inability to separate imagination and illusion from calculation and reality. Historians have noted Raleigh's dramatic flair and have stressed the imagery of the stage in the *History of the World*. There is little, except perhaps the exaggerations, that can be called original in this book.

Greenblatt has subtitled his essay "The Renaissance Man and His Roles." Presumably this is meant to underscore the fact that Raleigh played many parts. So did others. The book is not a biography, although it discusses Raleigh's life and work. It is not intellectual history, although it deals with Raleigh's ideas and attitudes. And it is not old-fashioned literary criticism, although there are some tedious examples of *explication de texte*. What is it? An uncharitable answer might be that it is a neatly packaged product of the Eng-Lit industry. The author is fond of departmental clichés—ambiguity, ambivalence, dramatic sense

of life, role playing, synthesis of the objective and subjective will, tragedy of imagination, and so on. These terms are like staples, fastening live emotions and thoughts to professional file cards. When Greenblatt discards the trappings of his trade he writes clearly and makes telling arguments. He is especially good at challenging Pierre Lefranc's brilliant but sometimes perverse criticism; Greenblatt argues convincingly that Raleigh's best known poem, "The Lie," was indeed by Raleigh.

An illustration must suffice to show what is lacking. Greenblatt argues that "the inner tensions and conflicting world views that we have examined in his poetry and his career reappear tied to his shifting interpretation of history." To a degree this is true, but only to a degree. There are obvious inconsistencies in the *History of the World*, but inconsistency is not necessarily evidence for any of Empson's Seven Types of Ambiguity. Raleigh's apotheosis of death at the end of the work is not, summarily, just a "bitter assertion of human emptiness." Raleigh's words are not so far from tradition as this thesis makes them appear. Raleigh wrote that "it is therefore Death alone that can suddenly make a man *to know himself*" (the italics are mine), and among other things, death "humbles" men and makes them "repent." By selectively ignoring various traditions and customs (especially medieval ones) upon which Raleigh drew, Greenblatt creates a literary model, not a living human being, much less a tragic figure set in his time and place.

To sum up in a sentence is to exaggerate, but historians should at least be cautioned: the author interprets Raleigh's life by his writings, not his writings by his life. Men of action are seldom as consistent as men of words would have them. Traditions themselves are apt to be ambiguous because they are shaped less by reason and logic than by custom and emotion. Raleigh was, like many practical men, at once a visionary progressive and a profound traditionalist.

F. SMITH FUSSNER
Spray, Oregon

BRIAN P. LEVACK. *The Civil Lawyers in England, 1603-1641: A Political Study*. New York: Oxford University Press. 1973. Pp. viii, 311. \$16.00.

A study of those men trained in civil law during the Tudor-Stuart period is long overdue. An examination of the attitudes and activities of this professional group is essential for an understanding of the circumstances that led to the conflict between king and parliament in seventeenth-century England. Brian P. Levack has attempted to remedy this deficiency by providing a study of "all those civilians who received their doctorates before 1641 and who resided in England between 1603 and 1641." Included among the two hundred civil lawyers who form the basis of this work are those who were incorporated at English universities after having been trained on the Continent. Attempting to explain the royalism of the civilians prior to the English Civil War, Levack has approached them as "officials in the King's governments, lawyers in financial difficulties, political theorists, critics of the common law, and defenders of the English Church." A biographical dictionary, consisting of thumbnail sketches of the civilians' careers and comprising almost one-third of the text, concludes the work.

Levack persuasively assumes that the royalist tendencies of the civilians must not be attributed to any one factor. Instead, he suggests that an interaction of ideas, economic necessity, and professional considerations must be taken into account. He argues that during this period civilians differed from common lawyers less regarding political fundamentals, such as royal authority, sovereignty, and resistance, than they did in matters that pertained to their professional advancement and success. The civil lawyers, Levack contends, did not have the luxury of independent incomes. When confronted with diminishing opportunities for employment in those courts that utilized civil law procedure, they necessarily identified their interests with the court and, more importantly, with the Anglican Church. Thus they became some of the staunchest supporters of the Stuart monarchy and the Laudian church.

However, questions may be raised about Levack's assumptions with respect to the composition of the civil law profession during this period and with regard to the thoroughness of his selection of civilians. The repeated warnings issued to recipients of only the baccalaureate degree in civil law, which attempted to

prohibit them from performing the functions of civilians, raise doubts as to whether Levack's selection indeed represents the political ideas and career objectives of the profession. Moreover, contrary to the author's assertion about the completeness of his study, a careful scrutiny of the matriculation registers apparently utilized by him reveals twenty-three recipients of doctorates in civil law who are not included in the work.

CECILE ZINBERG

California State University,
Fullerton

ROBERT W. MALCOLMSON. *Popular Recreations in English Society, 1700-1850*. New York: Cambridge University Press. 1973. Pp. x, 188. \$11.95.

Brian Harrison has written that "nineteenth century historians usually study social class in relation to work rather than leisure." Fortunately, there have been indications in the last few years that popular games and recreations, for too long the province of antiquarians, are finally being accorded the attention they deserve from serious social historians.

This short monograph consists of two parts. The first is a description of traditional, pre-industrial popular recreations, including bull-baiting, several early forms of football, wakes, fairs, and local holidays. Professor Malcolmson goes beyond mere description, however, and places these amusements into the social context of rural society. He suggests the ways that these recreations filled the social needs of laboring people, providing them with opportunities for self-expression, outlets for aggression, and even, on some occasions, camouflage for social and political protest.

These recreations, he notes, were deeply rooted in the natural rhythms of the agricultural year and could survive, despite some opposition, because they received the support of the gentry, many of whom "were not entirely disengaged from the culture of the common people" and accepted traditional recreations both as part of the natural order of things and as useful social tranquilizers.

The second part of the book describes the undermining of popular recreation in the late eighteenth and early nineteenth centuries by

such "modernizing" forces as enclosure, urbanization, evangelicalism, and the growing need for rigorous labor discipline. The picture presented by Professor Malcolmson here is less clear, though he is probably not to blame. Each of the modernizing forces he describes contributed to the elimination of many popular recreations, but the precise role of each may be beyond sorting out. There were regional differences, but the sources really do not allow for more than a somewhat impressionistic account of a general national trend. It may be, as Professor Malcolmson's account suggests, that the greatest factor behind the elimination of traditional rural recreations was the elimination of the countryside as the home of the majority of the population.

This reservation aside, Professor Malcolmson has made a welcome contribution to our understanding of the life—and death—of traditional society.

DAVID C. ITZKOWITZ

Macalester College

GEOFFREY BENNETT. *Nelson the Commander*. New York: Charles Scribner's Sons. 1972. Pp. xii, 322. \$12.00.

Nelson is a figure of perennial fascination. His victories, unorthodox private life, and melodramatic death created a legend that has enshrined him in popular memory and attracted a succession of writers, from Robert Southey on, who have built up a very formidable library of Nelsoniana. As a result there is little that we do not know about Nelson, his captains, his ships, or his mistresses, and the merit of new books about him must rest upon their literary quality or the freshness of the synthesis they provide, the archives having little left to yield. In recent years we have had a full-scale biography by Carola Oman and a shorter, more incisive "portrait" by Oliver Warner, not to mention individual studies of Nelson's battles by Warner, Dudley Pope, and David Howarth. In the background there stand the serried volumes of documentation published by Sir Harris Nicolas and the Navy Records Society, as well as Mahan's classic study of Nelson as a commander. Geoffrey Bennett does not really add anything new to all this, either in detail or interpretation. The book is certainly well

written and the publishers have been lavish with illustrations, diagrams, and maps. Bennett, who has several good books on British naval history to his credit, has not forgotten, as many writers do, that naval warfare under sail is a very arcane subject, and he has provided an excellent, concise account of the organization, administration, strategy, and tactics of the Royal Navy during the years when Nelson was winning his great victories. The description and analysis of the campaigns and battles is clear and convincing. Nor are the warts left out. The rashness that produced the fiasco at Turks Island in 1783 was repeated at Tenerife (1797) and Boulogne (1801). The humanity that held floggings to a minimum in the early stages of his career was not so noticeable at the end, when H.M.S. *Victory*'s log recorded an "appreciable number." The less pleasant side of Nelson's ambition, his vanity, is not glossed over, nor are the unattractive aspects of his relationship with Lady Hamilton. It is nice to see Lord Keith (commander-in-chief in the Mediterranean, 1799–1801) given full credit for a forbearance that many senior officers would not have shown even to a Nelson. Yet there is nothing in any of this that will be new to serious students of British naval history. This book is an excellent introduction to Nelson, rather than a must for scholarly readers.

RAYMOND CALLAHAN
University of Delaware

T. J. RAYBOULD. *The Economic Emergence of the Black Country: A Study of the Dudley Estate*. Newton Abbot: David and Charles. 1973. Pp. 272. £5.25.

Landowners for centuries in that area of Staffordshire and Worcestershire that came to be known as the Black Country, the Dudleys found that, when the pace of economic growth began to quicken in England in the later eighteenth century, they were sitting on the metaphorical gold mine. Starting in 1774 when the second Lord Dudley and Ward came into the estate, Mr. Raybould considers particular elements of the exploitation of their estates in turn. By enclosing their lands the Dudleys were able to exploit the mineral resources

underground to become the leading mine owners in the Black Country. Clay was used for brickmaking, and limestone, coal, and iron provided the raw materials for the iron works they established. The Dudleys also had a share in the improvement of roads, in the construction of canals, and in the building of both narrow-gauge local and standard-gauge main line railways necessary to permit the economic development of an area that had poor natural communications. While their principal efforts were devoted to industry, they also tried initially to farm their estates more efficiently. But Mr. Raybould argues that for the years 1774–1833, the period he considers in most detail, "the administration of the Dudley estates was not adapted to meet changing conditions, traditional practices remained and management was generally inefficient." Although the estates continued to be profitable in the later nineteenth century when the economic tide in the Black Country had already begun to turn, the prosperity did not continue, and, like many other aristocratic landowners, the earls of Dudley suffered from the legislation of the early twentieth century and the bleaker economic conditions after 1920. Further affected by the nationalization of coal and iron after the Second World War, the estates of the Dudleys in the Black Country had shrunk to but a few acres as the family turned from landowning to a new role, predominantly financial and *rentier*. Thus Mr. Raybould's account extends beyond the economic emergence of the Black Country into its decline, and it virtually ends with the death of the late earl in 1949.

More limited than the main title suggests, this book does not, on the other hand, provide an effective picture of the operation of the Dudley estate. The bits of the jigsaw are never fitted together. Although he has had access to the Dudley papers, Mr. Raybould was apparently not able to come across much in the way of direct evidence to show on what basis the Dudleys conducted their estates. In particular, the claim that the third earl of Dudley was probably the most successful aristocratic entrepreneur to survive into the twentieth century is largely unsubstantiated, while the earl himself, like his predecessors, whose relationships would have been illumi-

nated by a genealogical table, remains a shadowy figure.

WALTER E. MINCHINTON
University of Exeter

have precluded footnotes or an adequate listing of manuscripts.

JOHN E. CASWELL
*California State College,
Stanislaus*

ERNEST S. DODGE. *The Polar Rosses: John and James Clark Ross and Their Explorations.* (Great Travellers.) New York: Barnes and Noble. 1973. Pp. 260. \$9.25.

In the nineteenth century, polar exploration was the challenge that lunar exploration is currently. Two of the outstanding leaders of that period were Captain Sir John Ross and his nephew James, who later became a rear admiral. Dr. Dodge, director of the Peabody Museum in Salem, Massachusetts, has written the first book-length biography of the two men.

Captain John commanded the Admiralty's Arctic expedition of 1818 that restored Baffin Bay to the maps. James sailed as midshipman. On their return John ran afoul of crusty old John Barrow, second secretary of the Admiralty, who saw to it that he never received another naval Arctic command. James, however, sailed on two expeditions under Parry. The two Rosses were together again on a privately financed expedition in 1829-33, on which James reached the North Magnetic Pole. Sir James's most notable voyage was a magnetic survey in Antarctic waters in 1839-43, during which he discovered the Ross Sea, Ross Ice Barrier, McMurdo Sound, and Victoria Land. Both Rosses participated in searches for the lost Franklin expedition.

Dodge penetratingly observes that John was an eighteenth-century man in his outlook, controversies, loyalty, and diversity of interests. James, he concludes, was a sailor's sailor and a nineteenth-century man "in his reticent severity, his cool efficiency in his commands, his quiet competence in his scientific pursuits."

We can be grateful that Dodge undertook to cram the essence of two active lives into this small volume. The book will introduce the general reader to a series of most interesting expeditions. Historians will find it useful in giving the Rosses due credit for their substantial accomplishments. They will regret that the limitations of the Great Travellers series

ROBERT E. ZEGGER. *John Cam Hobhouse: A Political Life, 1819-1852.* Columbia: University of Missouri Press. 1973. Pp. vi, 312. \$11.00.

The intractable problem in writing historical biography is always that of the decision as to how much history should be included for the assistance of the reader. Perhaps in an effort to meet this difficulty Robert E. Zegger has subtitled his biography of John Cam Hobhouse *A Political Life, 1819-1852*. This definition enables Zegger to concentrate on the development of Hobhouse's political career. Nevertheless, the dilemma remains. If the experience of Hobhouse as parliamentary candidate and member of Parliament for Westminster is recounted, how much of the history of that unusual parliamentary seat should be provided? Zegger, in his introduction to this study, devotes over fifteen pages to the radical fortunes of Westminster from the time of Wilkes. Similarly, the author finds the treatment of Hobhouse and India tricky. This political life is rather an account of various themes in Hobhouse's life, his radical career, his activities on behalf of nationalist groups abroad, his work for the Select Vestries Bill of 1831, and his periods in office. The emphasis on each theme varies.

Despite certain problems of balance, this book makes a useful contribution to the history of early nineteenth-century radicalism. It adds a number of illuminating details, in particular to the passing of the 1832 Reform Bill. Zegger emphasizes from the beginning the basic difficulty for the historian interested in Hobhouse's career, the apparent inconsistency of his political opinions, his "trimming" to the political wind. In chronicling these shifts of opinion, he does not, however, succeed in explaining Hobhouse's swing from extreme radicalism to something like middle-of-the-road Whiggery. Zegger pinpoints the crucial role played by the campaign for parliamentary reform of 1831-32 in persuading Hobhouse of the undesirability of rousing violent mob action. The coincidence of the offer of ministerial of-

fice to him is not really assessed. Hobhouse may have been "bought off" to encourage his silence or have been recruited simply to placate the radicals.

In the absence of a biography of Joseph Hume, Zegger's account of Hume's activities in the raising of funds for Greek and Spanish rebels is particularly welcome. The dust jacket commentator expects that this book will be found "intriguing" by "Byron scholars and other scholars of nineteenth-century English letters and literature," but I fear they will be disappointed. Hobhouse's relationship with Byron is not explored extensively. According to the *Dictionary of National Biography*, Hobhouse is thought to have invented the phrase "His Majesty's Opposition." Zegger quotes (p. 104) Hobhouse's use in 1826 of the phrase in the Commons and then remarks that "Canning's and Tierney's use of the phrase confirmed the acceptance of opposition within the constitutional framework of nineteenth-century Britain." A footnote referring to the work of A. S. Foord is the reader's only evidence for such a brave assertion. The omission of date and place of publication for many of the works cited in the footnotes, as for instance on page 5, is very irritating but may have been controlled by the publisher. The full references are provided in the bibliography, but, even so, a university press should surely conform to accepted convention.

VALERIE CROMWELL
University of Sussex

THOMAS J. SPINNER, JR. *George Joachim Goschen: The Transformation of a Victorian Liberal*. (Conference on British Studies Biographical Series.) New York: Cambridge University Press. 1973. Pp. xii, 263. \$14.50.

Since Lord Randolph Churchill forgot Goschen he has seemed eminently forgettable. The two-volume biography by A. R. D. Elliot, which appeared in 1911, set forth with amplitude the steps by which the reforming Liberal of 1866 became a conservative Liberal in 1880, a Liberal Unionist in 1886, and, as a Liberal Unionist, a member of Salisbury's governments in 1887 and again in 1895. On the face of it there was no crying need for another biography, even after the passage of sixty-two years.

The author of this new life of Goschen

certainly has been assiduous in exploring the available papers of contemporaries, although it is not clear whether Goschen's own account of the mission to Constantinople (1880-81), which Elliot "somewhat abridged," was available to him. Nor has Spinner's reliance upon W. N. Medlicott's studies in the diplomatic history of the period produced a picture of Goschen the diplomat that is significantly different from that supplied by Elliot.

As a defender of laissez faire, Goschen perceived the Irish Land Act of 1881 as a "gigantic invasion" by government, but he appears oblivious of the long record of government intervention in the Irish economy. His own understanding of laissez faire permitted government intervention to penalize slum landlords, but not to subsidize public housing. While he understood very well the drift of Fabian thought, he ignored the changing conditions of business organization: the growth of corporate activity and the decline of rugged individualism.

Goschen's consistency as a Liberal Unionist and antisocialist comes across more clearly at some points in this new work than in Elliot. For example, Goschen's opposition to Chamberlain's tariff reform rested not only on the traditional free-trade conviction of many Liberal Unionists but also on his dread of protection as a step toward state socialism (p. 229). But there is nothing novel about the theme of the aging Liberal who cooperates with Conservatives. The work will probably be most useful to libraries lacking a copy of Elliot.

BARRY MCGILL
Oberlin College

W. J. GARDNER *et al.* *A History of the University of Canterbury, 1873-1973*. Christchurch: University of Canterbury; distrib. by Whitcombe and Tombs, Christchurch. 1973. Pp. 530. \$9.75.

Among the colonies of England in the nineteenth century, New Zealand was the most fortunate in finding patrons and advisers in the mother country. Each of the provinces had connections through its settlers with organizations or families who took a deep interest especially in its churches and schools to offer advice and (less often) material help. This was particularly true of the two provinces in the

southern island. Otago was founded by the Free Kirk of Scotland, and Canterbury, the last offspring of Edward Gibbon Wakefield, had the Church of England (in theory at least) as its creator and the Archbishop of Canterbury as the titular head of the Canterbury Association. What was even more important, the first leaders had the blessing and promises of help from Oxford and Cambridge. Because of natural advantages in the production of wool, these two provinces were securely established without the growing pains suffered by the northern provinces. The short-lived gold rush in the sixties benefited Otago in particular and made Dunedin the most beautiful city architecturally of New Zealand. The steadfast determination of the Scots everywhere in the world to forward education made the miracle of a university in 1869 possible. But a group of men in Canterbury began planning for a college or university in the sixties and were able to admit some students by 1874.

The man in Canterbury who did most for what seemed at times a lost cause was H. J. Tancred, a Rugby graduate who had not gone on to Oxford or Cambridge and did not therefore strive too hard to imitate in every detail the old English universities. Tancred was a politician rather than a scholar and was able to foil in the New Zealand Parliament the efforts of Otago to make itself the arbiter of university education in New Zealand. Tancred worked for a University of New Zealand that should have the right of setting examinations and granting degrees and of which Tancred himself was chosen the first chancellor. The University of New Zealand had no buildings and no permanent home, but the stage was set for the development of university colleges in all the provinces that would have the same standards of examination.

It was at this point that the English universities made their greatest contribution. London recommended one of its own faculty to teach science and engineering and Oxford sent out J. Middleton Brown, who had a brilliant record in classics and who fitted into the community in Christchurch to the satisfaction of everyone. Cambridge contributed C. H. H. Cook, a mathematician and scientist.

This volume deals with almost every aspect of the growth and influence of Canterbury Col-

lege (to become a university in its own right in 1951) and stresses such matters of interest as the admission of more women than any other college even at its opening, the erection of dormitories attracting students from distant provinces, and the high quality of some of the students graduating. The most famous is Ernest Rutherford (later Lord Rutherford), the leading nuclear physicist in England for whom Canterbury showed more appreciation than Cambridge before his outstanding reputation was established. In reminiscing in later years, he said that C. H. H. Cook had given him greater inspiration than any teacher he had had. It would seem that the relaxed and broad curriculum of his New Zealand alma mater was more conducive to original thought and ambitious research than the more rigid requirements of Cambridge, which paid little attention to his genius until his reputation was established in the world.

HELEN TAFT MANNING
Bryn Mawr College

KENNETH O. MORGAN, editor. *Lloyd George Family Letters, 1885-1936*. New York: Oxford University Press. 1973. Pp. x, 227. \$14.50.

Students of Lloyd George's career know only too well how rare are confidential letters from "the Goat." Lloyd George must have written letters to his colleagues, but few of importance have survived. This may have been because Lloyd George was essentially an oral communicator who preferred to deal directly with politicians over the breakfast table, rather than indirectly through letters. Up to now, the lack of such letters has forced historians to rely on the memoirs and diaries of those around him. These sources are variable in quality, and quite apart from being hearsay evidence, many have put unduly critical constructions on Lloyd George's motives. Now, for the first time, we have in this collection of letters to his wife firsthand evidence about Lloyd George's views throughout his career.

The most noticeable thing about the letters is that they seem to be vacuous and selfish. There is almost no mention of the arts or of intellectual, social, or economic currents. Religion appears, but only as an adjunct to political maneuvers; and politics comes down to

almost exclusively party matters, jostling for position, and electioneering. *Family Letters* thus provides ammunition to unfriendly critics of Lloyd George, and one might even say, as was said of Douglas Haig and others, that with the publication of this book Lloyd George has committed suicide many years after his death. This would be misleading, since Lloyd George was writing to his wife, and despite their deep quarrels over his sexual lapses they still shared many common views; Lloyd George did not have to state in so many words that he was defending nonconformity and trying to lift up the poor. It is only when the letters deal with new subjects that a false note sets in. There was no clear understanding between husband and wife over the outbreak of the First World War, and the phony sentimentality of Lloyd George's comments often sound as if he is trying to convince Dame Margaret when he is only partly convinced himself.

Family Letters deals mainly with Lloyd George's early career; it is especially illuminating on the Welsh background. On the other hand there is comparatively little material on the major crises involving Lloyd George between 1914 and 1931; but Dr. Morgan fills many of the gaps with excellent introductions to each section.

MICHAEL KINNEAR
University of Manitoba

ROSS TERRILL. *R. H. Tawney and His Times: Socialism as Fellowship*. Cambridge, Mass.: Harvard University Press. 1973. Pp. x, 373. \$15.00.

R. H. Tawney, the democratic socialist, is hard to pin down. A tough-minded saint, he was revered in England by most factions on the Left who agreed on little else, and his career was multifaceted: political thinker, academic, historian, gadfly to the Labour party, and author. The most influential and best-known of his books were *The Acquisitive Society* (1920), *Religion and the Rise of Capitalism* (1928) and *Equality* (1931). In them, and elsewhere, he continued in the line of Carlyle, Ruskin, and Morris, believing as they did in the moral necessity for a changed society but in a more evenhanded way. He was less emotional and less abstract than his Victorian predecessors—

or most English socialists—recognizing and respecting the actuality of the ordinary Englishman, whom he called “Henry Dubb,” neither romanticizing his nature nor abstracting his plight from the gritty realities of everyday existence. As a socialist he got beyond the limitations of guild socialism, at the same time as he recognized the dangers of too much power accruing to the state and the soul-limiting qualities of modern collectivism. On the whole, critics of society tend to be pragmatists or visionaries; Tawney was remarkable in being something of both, a practical thinker with a vision of what British society should be. Unfortunately he did not ever really enunciate, if such could be done, the exact program to achieve his goals.

Ross Terrill, in his study of this truly noble man, “blends history and biography with a study of his [Tawney's] ideas.” The blend, however, is achieved only through an adding up of the quite separate and distinctive parts of the study; a more historical, integrated treatment of life, times, and ideas might conceivably have proved more rewarding. As it is, the first sections consist of an extended biographical sketch—Tawney's Indian civil service family background; his education at Rugby and Balliol; experiences at Toynbee Hall; teaching in the North of England in the very earliest days of the Workers' Education Association; his marriage to William Beveridge's sister; his service as an enlisted man in the First World War, where he was severely wounded at the Somme. After biography comes analysis, and in his next section Terrill presents a clear, schematic, non-chronological discussion of the ideas that went into the making of Tawney's socialism, placing him in the tradition of political thought. Historical-minded readers may be disappointed that little attention is paid to Tawney's work in Tudor and Stuart history, but, as Terrill states candidly, he is not himself an expert in that field, and in any case, he makes very clear in what ways Tawney's conception of the earlier period was important for his thought. Terrill's own deep knowledge of China serves him admirably when he comes to Tawney's writings on China and provides him also with some intriguing comparisons with Mao. The last of the book is an assessment of Tawney's importance, followed by a bibliography listing 572

items of his published writings. Taken all together the result is a valuable study of Tawney's thought, enriched by a picture of the man, much of it based on interviews with those who knew him well.

PETER STANSKY
Stanford University

PETER DENNIS. *Decision by Default: Peacetime Conscription and British Defence, 1919-39*. Durham, N.C.: Duke University Press. 1972. Pp. xi, 243. \$9.75.

By 1932 the attitude of Germany and Japan forced the British government to look more carefully at its arrangements for national and imperial defense. There were bitter disputes about the best way to achieve national security. *Decision by Default* makes significant contributions to the literature of this complicated subject. The book deals with much more than "peacetime conscription and British defence." Based upon massive collections in the Public Record Office and other unpublished sources, it makes clear the dilemmas of British defense policy in the period between the wars.

When the need for rearmament made itself felt two schools of thought emerged among the planners. Upon the one hand were those who urged a policy of "limited liability." They knew Britain's resources were not boundless and had to be exploited as efficiently as possible. They wanted the government to build up the navy and air force, allotting a minor role to the army. This, they felt, would avoid serious injury to the nation's financial position, its "fourth arm of defence." Their opponents disagreed with this strategy. They held that the only way to render the country safe was to create an expeditionary force that could play a significant part in a European war and that could serve also as the basis for further expansion of the army. They advocated a "continental commitment" as the key to Britain's security, arguing that the British Isles could only be defended on the Continent.

It was Neville Chamberlain who played a curious and significant role in deciding the issue. He dominated the opinions of his colleagues, allowing his policy of appeasement to transform the strategic situation in Europe. After Munich the French saw the collapse of an

effective eastern front against Germany, and when thirty-five Czech divisions were lost to their cause it was demanded that Britain restore the balance of forces. There was fear that France might come to a separate arrangement with Germany rather than fight without the support of a large British army. It thus came about that Chamberlain, the champion of "limited liability," became the first prime minister in history to introduce compulsory military service in peacetime. It was a gesture to satisfy opinion in France and in Britain. The analysis of these terrific events in *Decision by Default* is acute and the narrative excellent.

ALFRED COLLIN
University of California,
Santa Barbara

CLIFFORD GULVIN. *The Tweedmakers: A History of the Scottish Fancy Woollen Industry, 1600-1914*. (David & Charles Library of Textile History.) New York: Barnes and Noble. 1973. Pp. 240. \$12.50.

Surprisingly few very good regional histories of the British textile industry have been published since the 1930s. Though not so massive as theirs, this monograph will stand with Herbert Heaton's on Yorkshire or Miss Julia Mann's on the West of England.

Using the methodology of traditional economic history Dr. Gulvin provides a lucid and balanced analysis of the emergence and decline of Scotland's mainland tweed manufacture, a small industry that in 1907 (just past its prime) accounted for seventeen per cent of the value of British woolen piece goods production. He delineates a geographically remote, factory-based industry reinvigorated by a fashion fluctuation, carried forward by mid-Victorian expansive conditions, and then caught out by foreign competition, product overspecialization, and organizational weaknesses.

The cloth types created in the 1830s fashion revolution have received extensive scrutiny elsewhere. But to many other aspects of tweedmaking Gulvin's painstaking research gives new definition. For example, the quantitative data for plotting growth are carefully reconstructed. And a chapter on the means of sustaining expansion fully exposes the industry's structure and operation in sections dealing with technology, labor, wool, marketing, and finance.

By the 1890s internal flaws aggravated the difficulties arising from foreign protectionism and rivalry. Gulvin provides a sound account of the latter and clarifies the internal problems, mostly stemming from the commitment to an exclusive market: overpricing, minuscule production runs, neglect of technical education, and the intensely competitive and fragmented industrial structure that put manufacturers at the mercy of cloth merchants. Pursuit of higher standards and new products brought some renewal before 1914.

A final chapter shows that the wages of Scottish woolen workers (except for designers and finishers who did better in tweedmaking) remained on a par with those of their English counterparts. Paternalism and the small scale of production relegated unionism to an ephemeral phenomenon.

Rather short shrift is given to the industry's decline after 1914. To appreciate its zenith we surely need more than a couple of paragraphs on its nadir.

Wary of "technique-based history" (p. 11) the author could nevertheless have strengthened the technological side of his study, for example, by closer investigations of fancy yarns or dyeing, some very critical technical elements. And contrary to pages 102 and 154, ring spinning was never commercially possible for woolen yarns in the nineteenth century. Despite these reservations, the volume is unquestionably the best treatment of its subject.

DAVID J. JEREMY
Westcliff-on-Sea, Essex,
England

JOHN SELBY. *Over the Sea to Skye: The Forty-Five*. New York: St. Martin's Press. 1973. Pp. 170. \$8.95.

In this volume John Selby, who has written extensively about military events and personalities of the nineteenth and twentieth centuries, turns his attention to the last full-scale campaign in Great Britain, a subject about which previous commentators have seldom been neutral. This book is evidently not designed for scholars. Printed on slick paper it is beautifully and lavishly illustrated with portraits, battle paintings, maps, and plans. The occasional footnotes are explanatory rather than precise references

to sources, and the bibliography, while helpful, is selective rather than exhaustive. It seems likely that this account is aimed at those of the public who desire to find out "what happened" and are attracted by the feel and look of the book.

Such a purchaser will have many reasons to feel satisfied. One of the virtues of this volume is its nonpartisanship. The actions of both sides are recounted without emotion, excuse, or advocacy. A certain amount of imagination is evident in the decision to start the story with the duke of Cumberland, describing his background and his role in the Battle of Fontenoy, rather than with his more romanticized cousin, Prince Charles Edward, but the book soon settles into a narrative with its focus on the prince and his activities following his embarkation from France. Cumberland reappears only briefly as a threat to the Jacobite army at Derby and, more extensively, as the commander of the victorious forces at Culloden. Selby's emphasis is almost exclusively on the military aspects of the adventure: plans, recruitment, marches, sieges, battles and, finally, escape from Scotland. The author gives a clear and chronological narrative of such events, but he provides no new details or interpretations of them, nor does he deal at length with any other aspects of the affair. No speculations are advanced about such questions as the probable fate of the Jacobite army if it had proceeded toward London (as the prince wished), the responsibility for repeated failures of organization and supply among the rebels, the relative capacities of the officers on either side, or whether there was ever any chance that Charles Edward could achieve his stated aims. Such lack of interpretation is especially serious in a work designed for nonspecialists, since it leaves them without any substantial context for the story and does not suggest to them any of the most interesting questions that can be raised about it.

J. WILSON FERGUSON
Russell Sage College

A. J. YOUNGSON. *After the Forty-Five: The Economic Impact on the Scottish Highlands*. Edinburgh: Edinburgh University Press; distrib. by Aldine Publishing Company, Chicago. 1973. Pp. xi, 246. \$15.00.

Professor Youngson has written a learned and

thoroughly documented book that covers much of the same ground as Malcolm Gray's *The Highland Economy 1750-1850*, but from a rather different point of view. Gray's groundbreaking and very useful book provided the first modern, detailed account of the economic development of the area after Bonnie Prince Charlie's rebellion; Youngson deals with the period from the perspective of the proposals put forward for the Highlands by the economic planners, notably James Anderson. The theorists believed in the great economic potential of the Highlands; they also subscribed to the view of Sir William Temple who, on the basis of his experience in the Netherlands, believed that a "great multitude of people crowded into small compass of land" is the cause of prosperity. So emigration from the Highlands, which began in a small way in the 1760s and 1770s, and worried contemporaries, was to be checked by the creation of industries, markets, and improved transportation. Youngson then recounts the attempts to develop such businesses as linen and fisheries and to create fishing villages (of which Ullapool was one) and to collect the tax money due the government from distilling, an industry whose growth required no encouragement. There was a brief, delusive prosperity in some parts of the west Highlands from kelp, which ended by the 1820s on account of the end of the war and the reduction in duties on competing sources of alkali. And there was the Caledonian Canal that was supposed to bring about all sorts of economic development, but that instead was "one of these conspicuous white elephants conceived by ambitious and ingenious engineers and enthusiastically brought to birth by misguided politicians" (p. 152). None of these schemes for commercial and industrial development had anything more than very partial success. The result was inevitable: emigration on a scale so large as to amount to depopulation. There were more people in the Highlands in the 1830s and 1840s than there ever have been since. The emigration is usually ascribed to the coming of large-scale sheep raising and the uprooting of tenants by ruthless, tight-fisted landlords, but, as Youngson points out, the clearances "were heralded . . . by fifty years of remorseless and extensive change" (p. 185). By the 1840s "the idea of building up the highland econ-

omy was over," not to be revived until our own time (p. 190). This is an excellent book. The author's style is felicitous and his analysis convincing. The illustrations are well chosen, but do not include a modern large-scale map, which would have been helpful.

MAURICE LEE, JR.
Rutgers University,
New Brunswick

T. W. MOODY and J. G. SIMMS, editors. *The Bishopric of Derry and the Irish Society of London, 1602-1705*. Volume 1, 1602-70. (Coimisiún Láimhscríbhinní na hÉireann.) Dublin: Stationery Office for the Irish Manuscripts Commission. 1968. Pp. 430. £6 6s.

The documents in this volume, selected from materials in the Public Record Office of Northern Ireland and the Guildhall Library in London, were compiled for a 1945-48 court case concerning the fishing rights of the Irish Society, the corporation initially founded for the colonization of Londonderry. The dispute originated from rival claims of the Society and the bishop of Derry over fisheries attached to tithes and *erenagh* lands, that is, former monastic and other Church holdings. The complexities of these ancient rights illustrate the tenacity of feudal arrangements. There is much on the temporalities of the see of Derry and on the bishops' relations with their lessees (often landed families) and other tenants as well as with the Society. Among the bishops involved was George Downham, whose survey of the diocese was published by the *Ulster Journal of Archeology* in 1894-95. Charles I, by means of a Star Chamber case, took over the Society's property and granted the disputed lands to Bishop John Bramhall in 1636. Over half of the volume deals with the period 1602-40. In addition to what has been indicated, this section contains information on the building of Londonderry and on rents and farming conditions as well as on schools (or their lack) and the state of the local clergy and churches.

After a few interesting documents on the outbreak of the Irish rebellion in 1641 there is a gap until 1657, when Lord Protector Cromwell, employing the royal "We," issued a new charter (given in full) to Londonderry. This recognized the Society's rights but of course excluded the bishop's. Much of the last quarter of the

volume is concerned with the restoration of the bishop's and other ecclesiastical property. Bramhall's successor, George Wilde, was a Londoner whose efforts to seek support from the London guilds composing the Society met with some success. Among other documents on the diocese after 1660 are two lists of incumbents for 1661 and 1669. The first gives the value of the livings and the names of the "Gentlemen" in each parish; the second lists the patron of each benefice.

The table of contents includes the documents to be published in the second volume (covering 1670-1705). When that appears it will undoubtedly contain an index to both volumes and, one hopes, a glossary of such terms as townland and quarters, both land measurements, that are peculiar to Ireland. The editors seem to have done a careful job; they provide a good, brief introduction but no explanatory footnotes. T. W. Moody is author of the key book, *The Londonderry Plantation* (Belfast, 1939), and J. G. Simms is an outstanding authority on Ulster in the late seventeenth century.

FRANCIS G. JAMES
Tulane University

DESMOND SEWARD. *Prince of the Renaissance: The Golden Life of François I.* New York: Macmillan Publishing Company. 1973. Pp. 264. \$14.95.

This is a handsome biography. With its numerous illustrations and boldly printed text, it has a coffee-table-book look that is enhanced by an absence of the visual clutter of academic footnotes. That the work should have a format so pleasing to the eye is appropriate, for Mr. Seward is at his best as a historian of appearances. Fashions and castles, ceremonies and processions, diplomatic encounters and the battles of love and war are described in considerable detail, with the chronology of Francis's life serving as a compositional framework for evidence that has been drawn from published journals, memoirs, ambassadors' reports, and chronicles, supplemented by occasional purple passages from the works of writers such as Michelet, Strachey, and Belloc. The result is an entertaining narrative of the major events of the reign, accompanied by word portraits of

Francis and other illustrious persons of the principal courts of Western Europe. It is unfortunate that many of the portraits are reduced to caricatures, such as the one-liner on Juana of Castile, who was "so mad that people said she ran up curtains like a cat."

The central argument of the book, stated with typical exuberance, is that "no ruler since Charlemagne has had a more direct influence upon the civilization of France" than Francis I, whose greatness "lies in his role as supreme patron of the later Renaissance." A great patron Francis certainly was, and the importance of his patronage in the cultural history of Renaissance France has long been recognized. However, Seward's descriptive listings of the *objets d'art* acquired for the king's collections, the building projects undertaken at royal residences, and the literary works produced during the reign do not clarify the extent to which Francis personally understood or was committed to the innovations of mannerists and humanists in his service.

Even if one agrees that royal patronage under Francis I helped to bring about major cultural changes, the notion that the true significance of the reign is found in the spectacle of the king "riding, dancing, or promenading in an unending pageant, escorted by glittering nobles, by famous poets, painters, and savants," is difficult to accept. The reign was indeed one of the most brilliant in the history of France, but an abundance of published sources and secondary sources not used by Seward clearly indicate that the period was also one of fundamental social, economic, and institutional change. Until a history of the reign appears that incorporates the results of recent scholarship in these areas, it is not likely that the study written by Terrasse a quarter century ago will be superseded.

LLEWAIN SCOTT VAN DOREN
Boston College

RENÉE KOGLER. *Pierre Charron.* (Histoire des idées et critique littéraire, volume 127.) Geneva: Librairie Droz. 1972. Pp. 182.

Pierre Charron was a moral philosopher whose chief work, *De la sagesse* (1601), was an attempt to teach men how to master their passions and achieve a skeptical inner wisdom in the after-

math of the agonizing choices of the French religious wars. Borrowing widely from Montaigne, Du Vair, Lipsius, and Bodin, as well as from Seneca and the stoic philosophers, he produced an awkward, eclectic treatise that was popular in the next generation because he had combined a number of topical intellectual currents into a didactic, systematic argument. Charron, a Catholic priest, divorced his secular moral system from all religious considerations; expressed skepticism about knowledge and rational thought; endorsed the absolute authority of the prince; and offered an antirationalist defense of faith. Despite the ambiguities and contradictions throughout the book its elements were absorbed by a paradoxical diversity of groups: the free thinkers picked up Charron's implied separation of morality from religion; religious reformers, especially Jansenists, liked his skepticism toward the possibility of rationally knowing God. Charron anticipated Pascal's wager and some of the elements of Descartes's systematic doubt. He was admired by Gassendi, Naudé, and La Mothe Le Vayer; Saint Cyran defended his book; Bayle and Rousseau were influenced by it.

Yet despite all this Charron remains a secondary figure, a mere transmitter of ideas. Kogel has written an apology of her subject, designed to rescue him from the charge that he was just a disciple (or even plagiarizer) of Montaigne. On this point she is convincing: Charron borrowed from a variety of sources and his synthesis was his own. But her detailed study, the most thorough in English, tells us little of importance that was not adequately summarized in the works of Sabrié, Adam, Popkin, Church, Busson, and Rice. It tediously analyzes each aspect of Charron's thought in chapters fragmented by far too many quotations in French. Readers will not be convinced that Charron was original enough to merit another whole monograph, or that *Sagesse*, however popular, deserves to be removed very far from the shadow of Bodin and Montaigne; in fact the evidence of its *separate* influence is thin. Thus even in a work devoted to them Charron's ambiguities seem pale when set against the backdrop of other thinkers who were struggling with the implications of secularism. Perhaps by concentrating more on this broader influence

Kogel might have rescued Charron's reputation more effectively.

WILLIAM H. BEIK
Northern Illinois University

FRANCES ACOMB. *Mallet Du Pan (1749-1800): A Career in Political Journalism*. Durham, N.C.: Duke University Press. 1973. Pp. xii, 304. \$11.75.

The Swiss-born Jacques Mallet Du Pan customarily shares a historical footnote with fellow survivors of the Monarchien group that broke with the patriot majority in the summer of 1789; or else, as a counterrevolutionary publicist and intelligence agent, he is associated with Gentz and Burke. By way of contrast Professor Acomb reaches back two decades prior to 1789 in uncovering Mallet's evolution as a journalist, historian of recent events, and political theorist. She shows that, rather than representing the climax of Mallet's intellectual and professional life, the 1790s served as the denouement. Mallet Du Pan was an antirevolutionary before the Revolution. Formed in the volatile political and social climate of Geneva of the 1760s and nourished upon the fashionable Voltairism of the Négatifs, Mallet broke with the philosophes and factional politics in what Acomb calls an assertion of personal autonomy. Hers is a legitimate position to take. Mallet is a puzzling figure, a paradoxical one. Empiricist, humanitarian, and reformer, he desired religious toleration, opposed much corporate privilege, and instinctively supported the underdog. At the same time, however, he could not stomach what he considered the moral hedonism of Voltaire, d'Alembert, and Condorcet, and in the end his antidemocratic bias caused him to turn upon Rousseau. Perhaps the most important influence upon his life and work was an even more lonely figure, that brilliant, erratic bundle of contradictions, Simon Linguet.

Because he was among the first to make a lucrative career in journalism, especially as political editor for Panckoucke's *Mercur de France*, Mallet appears to have little in common with the miserable hacks, pamphleteers, and pornographers who belonged to the counterculture of letters in France during the 1780s. Like them, however, Mallet held the old monarchy in contempt while chafing at insti-

tutionalized restrictions upon liberty of expression. Acomb's study would have profited from exploring Mallet's Paris career in wider perspective. We learn precious little about journalism in the capital. Had she gone more deeply into press-government relations in the early 1780s, particularly with regard to imported books and periodicals, Acomb might not have underestimated the protectionist reasons lying behind Vergennes's hostility toward Mallet's first journals, the Geneva-based ones. She appears uncertain as to the various categories of permissions awarded by the French government to journalists and authors. And from what we know in other instances, her generally positive view of the press lord Panckoucke ought to be nuanced. Nevertheless, this study of Mallet's career is commendable, not the least for its skillful use of archival sources in Geneva and Neuchâtel.

RAYMOND BIRN
University of Oregon

COLIN LUCAS. *The Structure of the Terror: The Example of Javogues and the Loire*. (Oxford Historical Monographs.) New York: Oxford University Press. 1973. Pp. xv, 411. \$29.00.

Claude Javogues arrived at Saint-Etienne in mid-October 1793 fresh from the republican victory over besieged Lyon, thirty-two miles to the northeast. Like dozens of other representatives sent by the National Convention on missions, he was authorized to command any actions he deemed necessary for the salvation of the beleaguered and strife-torn Republic. He remained in the Loire *département* until early November and returned there in mid-December. By mid-February 1794 he had aroused vociferous local opposition, especially in Roanne, and alienated his colleagues within the Convention, even Collot d'Herbois. He was recalled to Paris under threat of arrest.

The political history of the Loire *département* during Javogues's mission is an illustrative episode in the development of the revolutionary government of Year II. It is fully analyzed in this excellent book, which is based on mastery of diverse and voluminous documentary evidence and an admirable familiarity with the local geography, economy, and social system; the period; and the political ac-

tors. Every chapter is dense with information and illuminated by suggestive insights. It is a demanding book; the more you already know about the topic, the greater your interest will probably be.

Lucas's view is that even the "anarchic" phase of the Terror was, in this *département*, a period of order compared with 1792-93 or 1794-95. His central chapters, two-fifths of the work, show the coherence of the governmental system by surveying the local political institutions: clubs, surveillance committees, the *armée révolutionnaire*, the commissioners moving about on revolutionary business, the ordinary administrative bodies, and the revolutionary tribunal at Feurs. This survey is preceded and followed by chapters on Javogues: first a fine account of his antecedents and political career; then an analysis of his role as representative on a mission, not only as the intermediary between the national government and the province, but also as the main source of energy for revolutionary change in the province. The latter chapter is followed by a long discussion of the terrorists, the local men on whom Javogues had to depend. Lucas produces much evidence for his conclusion that "the Terror was characterized by the transfer of power at the local level towards the lower end of the ruling class of the previous years" (p. 323). Militancy did not originate in conflict between classes, Lucas says; rather, it was the product of a series of political choices, each leading or predisposing to the next. The interpretation is concordant with the idea that revolutionary change was a vast and complex process of social learning.

Lucas's views are convincing. His (like any) perspective has its own limitations. The system of political institutions was not the only structure involved. Lucas has much to say about oppositions between town and countryside, between plains and mountains, between social classes, and between contrasting attitudes or beliefs, but he does tend to treat these as independent variables and to invoke them here and there as explanatory factors. His knowledge of local differences between districts (Saint-Etienne, Montbrison, Boën, Roanne), and his quantitative information, might have been used more methodically, though this would have required sacrificing some of the

valuable concentration on political institutions and personalities.

PHILIP DAWSON
Brooklyn College,
City University of New York

PAUL GONNET. *Un grand préfet de la Côte-d'Or sous Louis-Philippe: La correspondance d'Achille Chaper (1831-1840)*. (Analecta Burgundica.) Dijon: Société des Analecta Burgundica. 1970. Pp. 328.

Achille Chaper (1795-1874) was a product of the École Polytechnique, but resigned his commission in 1816 and moved into metallurgy in his native Isère. From 1820-28 he was also mayor of Pinsot, but with the coming of the July Monarchy he began a notable career as a departmental prefect, serving briefly in the Tarn-et-Garonne, then in the Gard, arriving late in 1831 in the Côte-d'Or through the intervention of Casimir Périer to whom he was distantly related by marriage. Chaper would subsequently hold two more posts between 1840 and 1848 and be elected deputy from Côte-d'Or in 1849. His public career ended with the coup d'état of 1851.

These letters reflect his prefectural career in the Côte-d'Or. Chaper left copies of only those letters he considered important. Paul Gonnet has selected 239 letters from the 3,000 that now rest in the Archives départementales de la Côte-d'Or, the documents having originally been part of a larger collection (see R. Avezou, *Inventaire des documents de la collection Chaper du château d'Eybens* [1953]) assembled by Chaper's son Eugène, himself a well-known Orleanist and a member of the parliamentary commission that investigated the Government of National Defense after 1871. The letters are arranged chronologically by topic. Political matters are separated from social and economic issues, and a detailed table of contents offers easy access to such topics as municipal elections; subversive activities; the surveillance of republicans, legitimists, and Bonapartists; relations with the notables; the diocese of Dijon; and the economic crisis of 1832.

In sum this is a well-edited source book, supplied with notes, bibliography, and index, for the specialist on the July Monarchy or the

student of French local government in the nineteenth century.

ROGER L. WILLIAMS
University of Wyoming

JEAN-ANDRÉ TOURNERIE. *Le Ministère du Travail (Origines et premiers développements)*. (Temps de l'histoire.) Paris: Éditions Cujas. [1971.] Pp. 447. 80 fr.

This book is first the history of an idea; then, the history of an institution embodying a modest part of that idea. Tournier's dissertation traces the idea of a ministry of labor from its appearance in 1848 until Clemenceau's decree establishing the Ministry of Labor and Social Insurance as part of his first cabinet, in 1906, and follows its functioning until 1914.

Tournier has had to rely chiefly on printed sources, especially parliamentary documents and debates and the press, since he turned up few of the manuscript sources he had hoped to find in the ministry's archives. The brevity of the period during which he observes the ministry in action is not compensated for by intimacy of the picture. There is little here, for example, on the role of one of the most remarkable of the *grands commis* of the Republic, Arthur Fontaine, a man appreciated both by employers and by reformist labor leaders, a friend of leading politicians and of great musicians and writers and painters. His work at the Ministry of Labor, said Daniel Halévy, "weighed more on the constitution of French society than did that of vague figures like Sarrien whose names encumber the chronicle of history." It still remains true, as Halévy suggested (*Decadence de la liberté* [1931]), that one needs to know the details of the work of a man like Fontaine to "know how a law was elaborated, drafted, and voted under the Third Republic." The parliamentary sources and the press, valuable as they are, cannot substitute for missing administrative archives and personal correspondence.

Professor Marcel David's introduction praises his student's work, but then deplores its "neutrality of tone" and its failure to realize Professor David's hope that it would "penetrate to the heart of the labor movement." What other tone would have been more appropriate to a study that begins with some of the warm-

est but least realistic proposals for workers' emancipation (the proposed "Ministry of Progress" of 1848) and goes on to describe a government bureau, later a department, navigating between the hopes of workers and social reformers and the fears of employers and reactionary politicians like Méline (whose fears of the ministry's action were at least as unrealistic as the generous hopes of 1848)? The attitudes of labor leaders and of workers of the syndicalist "heroic period" toward various forms of government intervention were complex and often contradictory; this study does not attempt to get at that aspect of "the heart of the labor movement." Of course the ministry of those years did not live up to the hopes of its friends any more than it did to the fears of its enemies. Tournier shows its most energetic efforts in its attempts to implement the abortive pension law of 1910, which the Confédération Générale du Travail and many employers opposed. Meanwhile spectacular government action against strikes alienated the labor movement.

The contrast between the political democracy of the Third Republic and its backwardness in labor legislation and industrial relations is, once more, apparent in Tournier's study. A single example: the Chamber of Deputies year after year begrudged the ministry and added a mere two more inspectors; in 1914 imperial Germany had 572 labor inspectors, while republican France had only 144.

VAL R. LORWIN
University of Oregon

W. H. C. SMITH. *Napoleon III*. New York: St. Martin's Press, 1973. Pp. 296. \$14.95.

The aim of Professor Smith's book is, according to the jacket, to "rehabilitate a major European figure who until now has not received a true measure of historical justice." The Second Empire, he says, "appears to tap a particularly bilious duct in historians." Ever since Victor Hugo's original hatchet job historians have been content to follow the Hugo line and to "perpetuate a mythology" about Napoleon III.

Smith's enterprise, it seems to me, rests on a faulty premise. Although the author has read extensively in the secondary literature and has dipped into the archives he has chosen for some

odd reason to ignore many of the modern historical works that have portrayed what Alan Spitzer calls "the good Napoleon III." Albert Guérard's literate and impassioned defense of the emperor might never have been written; neither it, nor Guériot's detailed biography, nor Bury's judicious synthesis is even listed in the bibliography.

But even if the profession had been thirsting for a fresh and sympathetic reappraisal of a misunderstood statesman this would not be the book to fill the need. Its merits, in my judgment, are heavily outweighed by its faults. The writing is often careless, the language ambiguous, the syntax awkward. Far too often, dubious assertions are advanced as fact with little or no supporting evidence. More than once the author appears to contradict himself. On some important and complex issues (e.g., the class basis of Napoleon's support, or the nature of the liberal reforms of 1870) the argument is more confusing than enlightening. Some episodes are quickly passed over without adequate explanation; others are spun out at excessive length. This imbalance is made more obvious by Smith's interest in diplomatic history, which leads him into secondary byways and thickets that clutter the story. On top of all this Smith has been badly served by his publisher. A careful editor might have corrected many infelicities and ambiguities; a good proof-reader would surely have caught the innumerable typographical errors.

Perhaps this review proves at least one of Smith's points: that books on the Second Empire "tap a particularly bilious duct in historians." Or perhaps any subject treated in this manner would start the bile flowing.

GORDON WRIGHT
Stanford University

BERNARD LE CLÈRE and VINCENT WRIGHT. *Les préfets du Second Empire*. (Cahiers de la Fondation nationale des sciences politiques, 187.) Paris: Armand Colin, 1973. Pp. 411. 107 fr.

This study of the 220 prefects who served Louis Napoleon during the Second Empire will interest specialists in the political and administrative history of that regime. Wright and Le Clère have joined their respective talents in

a work characterized by thorough research, erudite description, and clear topical organization.

A brief preliminary section discusses the impact of the coup d'état and repression (1851–52) on prefectural political functions and personnel. The second section reviews the subsequent history of prefectural power, arguing that administrative and political limits on the prefects were far more significant than critics have admitted. The third section presents evidence on the social and geographical origins, educational backgrounds, early careers, and political antecedents of the prefects. Here the authors suggest that administrative ability rather than wealth, family prestige, or political affiliation was the major criterion for recruitment and promotion within the prefectural corps, especially in the 1860s. They relate the trend toward “professionalization” and “bureaucratization” to that larger movement of “depolitization” that reshaped prefectural functions as the regime drew to a close. Two further sections present disparate materials on prefectural life styles, administrative routines, incomes and expenditures, retirement benefits, and job insecurity, while a final section describes the fate of individual prefects once the Second Empire collapsed. Extensive appendixes and a fine bibliography complete the book.

The author's major thesis concerning the growing professionalism and declining political authority of the prefects is surrounded by so many biographical details that the reader is left more with an encyclopedic impression than with a persuasively developed argument. Their minute attention to the imperial prefects also lacks any broader comparative perspective on bureaucratic recruitment patterns, promotional practices, and functional roles. For example, if the social background of imperial prefects had been compared with that of all the sub-prefects, or with that of prefects in previous or subsequent regimes, the reader would have a basis for evaluating the significance of noble status and family wealth as particularistic criteria of appointment and advancement. Similarly comparisons with other European bureaucracies of the period would have established a standard for examining the relationship between educational performance and bureau-

cratic recruitment during the Second Empire. In their concern to rehabilitate the reputation of Louis Napoleon's prefects the authors may have underestimated the importance of social snobbery, oligarchical influence, and political favoritism within the prefectural corps. Only comparative analysis can provide a satisfactory answer to this problem.

TED MARGADANT
University of California,
Davis

PIERRE MIQUEL. *La paix de Versailles et l'opinion publique française.* (Nouvelle bibliothèque scientifique.) Paris: Flammarion, Éditeur. 1972. Pp. 610.

Miquel analyzes French opinion during the negotiation of the peace treaty of 1919. At the start of talks in January the French people had high expectations. On the one hand pacifists and socialists extolled President Wilson and his proposed international organization. On the other hand most Frenchmen were convinced that the treaty would provide for massive German reparations and for concrete and permanent guarantees of French security against future German aggression. News of the negotiations gradually destroyed both illusions. With the completion of the treaty in May it was clear that the French had lost the peace.

The revelation is how little the French protested. Maréchal Foch, in spite of his prestige, was unable to arouse opinion against Clemenceau's abandonment of the Rhine as the permanent line of French defense. The parliamentary debates over ratification were lackadaisical and were largely ignored by the press. The treaty was not an issue in the parliamentary elections of November. Miquel offers several explanations: preoccupation with domestic problems, the secrecy of the negotiations and the government's censorship and manipulation of the press, faith in the intransigent patriotism of Clemenceau, the reassuring despair of the Germans over the provisions. At the time of ratification the French were both unaware of the full extent of France's defeat at the peace table and resigned to France's inability to impose its views on its more powerful allies. The growth in 1919 of this attitude of resignation,

Miquel suggests, shaped the next two decades of French history.

But Miquel is unable to specify the real impact of the peace of Versailles on French public opinion. First, he can have no direct access to the attitudes of most French people. He focuses his research on the public's primary source of information, reading and classifying the articles in fifty-one Parisian and eighteen provincial daily newspapers, as well as in fifty-two weeklies and monthlies. But, as he admits, even this formidable accomplishment does not bring him to the level of popular *mentalités*. Second, the time span of his study is too limited. His detailed discussion ends with the completion of negotiations. Even if press coverage of the ratification proceedings was minimal and parliamentary candidates ignored the treaty it would seem impossible to appreciate the treaty's impact without analyzing what was said and not said at the conclusion of the whole affair. Furthermore a full appraisal would consider responses to later events such as the American rejection of the treaty and the French occupation of the Ruhr. Miquel's study is an invaluable first step.

The book is skillfully organized, but is long and repetitious. It is designed for specialists.

PAUL C. SORUM
University of North Carolina,
Chapel Hill

E. A. KRAVCHENKO. *Narodnyi front vo Frantsii, 1934-1938* [The Popular Front in France, 1934-1938]. (Akademiia Nauk SSSR, Institut Vseobshchei Istorii.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 294.

During the past twenty years Soviet historians have devoted considerable attention to the period of the mid-1930s when the USSR was temporarily aligned with liberal and socialist elements in the West against the common enemy of fascism. Most of this Soviet literature has treated the more dramatic episode of the Spanish Civil War, but Western analysts like Max Beloff have long argued that the key element in Soviet calculations was the triumph of the broadest possible "anti-Fascist" bloc in France. Madame Kravchenko's unusually candid remark that "the civil war and intervention in Spain complicated the activity of the Popular Front," as well as her stress on French Commu-

nist efforts to include Catholics and other non-socialist elements in a French Front imply, if they do not wholly concede, the validity of this interpretation. Her quotation of Pietro Nenni's condemnation of Trotskyite agitation for a worker-peasant government in France is entirely in line with the position of putting limited objectives ahead of revolutionary millennial goals. Fidelity to this interpretation tends to conflict, however, with the author's avowed aim of using the Popular Front as a model for contemporary action: "In our day, too, the Popular Front retains its attractive power for the international worker and Communist movement." Collaboration "from above" with official social-democratic parties (Kravchenko briefly but bluntly rejects Stalin's earlier "unfounded" vilification of "Social Fascists") may fit both approaches. The alternative interpretation, however, emphasizes violent class conflict (as in the French uprising of May 1968, which Kravchenko explicitly cites) even at the expense of alienating middle-class elements.

The preceding paragraph suggests that the principal value of Kravchenko's work is its revelation of contemporary Soviet interpretations. This is true, I believe, because the author virtually confines her treatment to narration based on contemporary newspapers, memoirs, and the few published French documents. While she utilizes some major new economic histories (e.g., Alfred Sauvy's) she makes little effort to re-examine the impact of economic developments on social groups, voting patterns, or latent group influence in French politics. She does not even cite old semi-Marxist analyses of this kind like Augustin Hamon's, to say nothing of reinterpreting the wealth of data that recent French political scientists and sociologists have provided. This retreat from Marxist sociological analysis to rather old-fashioned narrative description is, of course, characteristic of Soviet historiography. With certain exceptions on Middle Eastern subjects straight chronological narration predominates especially in Soviet treatments of recent developments abroad. In Kravchenko's case the limitation is compounded by her failure to take into account some important interpretations presented by non-Communist writers (notably Franz Borkenau and A. Tasca [Rossi]) although she does use some strongly anti-Communist

sources. More surprising is her failure to cite Ilya Ehrenburg's recent extensive eyewitness accounts, despite the fact that she cites one of his minor *Izvestia* articles from the 1930s.

JOHN A. ARMSTRONG
University of Wisconsin,
Madison

JOHN E. DREIFORT. *Yvon Delbos at the Quai d'Orsay: French Foreign Policy during the Popular Front, 1936-1938*. Lawrence: University Press of Kansas. 1973. Pp. xi, 273. \$10.00.

This account of Popular Front diplomacy makes clear that Yvon Delbos, foreign minister from 1936 to 1938, was a man of very limited vision, thrust into prominence during the political turmoil of the Blum ministry. It is very likely that he did represent a large segment of French opinion on international affairs since he continued in office under Chautemps. He had two basic diplomatic principles: a devotion to the concept of collective security and a determination to bring about the closest possible relations with Britain.

According to the author these two principles led Delbos into serious diplomatic errors. In his desire for collective security he mistakenly attempted to recreate a viable alliance with the Little Entente, which gained nothing for France. More serious, according to Dreifort, was Delbos's failure to respond to Russia's diplomatic overtures. This would have seemed a logical policy for France to pursue, but, as Dreifort explains, the French general staff greatly underestimated Russia's military capabilities. Also Delbos's anti-Communist feelings were intensified by the role Stalin was playing in Spain, by the wave of Communist-led strikes in France, and even by the attacks on him by *L'Humanité*, which Delbos believed were instigated by the Soviet Union. In addition the British were cool toward such an alliance.

The fact that France followed Britain's lead in international affairs was again apparent when the Spanish Civil War began. Both Delbos and Blum wanted to intervene, but because of pressure from the British they decided against it. Dreifort introduces new documentary evidence for this interpretation, quoting an unpublished note from the British am-

bassador to France that made it emphatically clear that England was against intervention.

Delbos did make one attempt at an independent French policy by organizing the Nyon Conference at which the signatory powers agreed to patrol the Mediterranean to force Italy and Germany to comply with the non-intervention agreement. The author maintains that, contrary to most interpretations, France did take the diplomatic initiative in this instance and was not merely following in the wake of British policy. Even after Nyon had clearly failed, however, Delbos continued with his policy of strict nonintervention, and France relapsed into her role of diplomatic subordination to Britain.

Dreifort declares that France gave up her freedom of action in exchange for an assured position as Britain's junior partner. He also states that historians have tended to overlook Delbos's genuine efforts to pursue an independent policy. But perhaps this point has been overstressed because even the Nyon Agreement was undertaken with the approval and support of the British, and it was at Britain's insistence that Italy was made a party to the agreement.

This book does not substantially change the depressing picture of appeasement policy in the 1930s. The author has made good use, however, of newly available British Foreign Office papers. He has also conducted personal interviews with colleagues of Delbos and has drawn upon the resources of the Association les Amis d'Yvon Delbos, which collects and publishes memoirs about the foreign minister. In sum it is a well-documented, scholarly study and a welcome addition to the diplomatic history of the interwar years.

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JULES JEANNENEY. *Journal politique, septembre 1939-juillet 1942*. Edited by JEAN-NOËL JEANNENEY. Paris: Librairie Armand Colin. 1972. Pp. xix, 514. 65 fr.

The *Journal politique* of Jules Jeanneney (1864-1957), president of the French Senate, is an indispensable primary source for the history of the last days of the Third Republic and the Vichy regime. Jeanneney, a highly re-

spected and well-informed parliamentarian, recorded his observations and conversations with political leaders from September 1, 1939, through July 9, 1942, except for a few lapses. The *Journal* has been edited with meticulous care and objectivity by his grandson, initially as a doctoral thesis (third cycle). The work includes a perceptive introduction, voluminous annotations, an extensive index, and seven appendixes containing a short biography of Jeanneney, his notes defending the prerogatives of the Senate and his own actions, his recommendations to General de Gaulle in the summer of 1942 on the future constitution of France, and his deposition before the High Court of Justice in 1945. A bibliography of works cited in the footnotes would have been useful.

Much of the story is familiar, but the journal provides new evidence, penetrating and often highly critical comment on the politicians and generals, and a portrait of Jeanneney himself, a bourgeois lawyer from Haute-Saône, who served forty years in parliament, but refused to accept any ministerial post except that of under-secretary of state in Clemenceau's cabinet during the First World War. Jeanneney's judgments were generally sound although he was mistaken in his first appraisal of Pétain and only belatedly recognized de Gaulle's potentialities. He hated the Germans, opposed the Armistice, and favored moving the government to Algeria. He criticized the Center and Right for not defending the Republic, but was himself attacked for lack of leadership, praise of Pétain, abstention in the crucial vote on the grant of constitutional powers to the Marshal, and failure to recognize opposition deputies. Jeanneney held that Pétain was the symbol of national unity, that a presiding officer should abstain, and that the Marshal's supporters drowned out the opposition. His poor health, legalism, horror of civil war, and lack of aggressiveness prevented him from assuming leadership, but he bitterly attacked the Vichy government and tried to defend Mandel and Jewish senators. As minister of state in de Gaulle's provisional government in 1944-45 he made constructive proposals concerning constitutional procedures.

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HERBERT TINT. *French Foreign Policy since the Second World War*. New York: St. Martin's Press. 1972. Pp. xi, 273. \$12.95.

A history of French foreign policy since the Second World War must be to some extent an exercise in imagination. The government archives remain closed. Statesmen rarely keep their papers and even more rarely grant access to them. Memoirs of participants are more often than not *ex parte* arguments with greater literary than historical merit. Books purporting to reveal the "secrets" of state mix the true with the false in proportions impossible to determine. And newspapers and periodicals disclose less about the actual process of policy formation in France than in any other major Western country. Under such circumstances the most one can expect from a book on this subject is a plausible collection of the miscellaneous facts available, ordered with good judgment and given coherence by the use of structural categories drawn from political science.

Within these modest limits Tint contributes a highly serviceable primer, the first in a series of Foreign Policy Studies to be issued by St. Martin's Press. Tint's work is not as comprehensive as previous entries into the field by Alfred Grosser and Guy de Carmoy, but is markedly less ethnocentric. He never loses sight of the fact that France has become a middle-sized power with essentially regional concerns, whose pretensions to a wider role in world politics are increasingly anachronistic and peripheral to its vital interests.

This perception logically impels Tint to focus on France's relations with Germany, its other Common Market partners, and the Soviet Union. The Indochinese and Algerian wars receive relatively cursory attention. This is unfortunate because France's efforts to play at great-power diplomacy elsewhere are more comprehensible when viewed against a backdrop of defeat and humiliation in these wars and the national trauma occasioned by the failure to save much, besides cultural prestige, from the process of decolonization.

Tint is particularly good, on the other hand, in showing how it rankled the French to be dependent on American economic assistance for a full decade after World War II and how pent-up resentment fueled their subsequent determination to extrude the United States from

a leadership position in Western Europe. Without adding anything new Tint presents a lucid summary of the development of West European integration. He attributes periodic obstruction by de Gaulle and his predecessors primarily to old-fashioned nationalism and a desire to ensure that France dominate United Europe.

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MIGUEL ARTOLA. *La burguesía revolucionaria (1808-1869)*. (Alianza Universidad. Historia de España Alfaguara, vol. 5.) Madrid: Alianza Editorial Alfaguara. 1973. Pp. 434.

Miguel Artola's book belongs to a new series on the history of Spain, of which he is the general editor. The series aims to give heaviest coverage to the recent past—an innovation for Spanish historians—and to emphasize social and economic structures, following the examples of Jaime Vicens Vives and the *Annales*.

This is Artola's finest book, perceptive, balanced, written in the tempered tone of a mature scholar. Many sections bring new clarity to the confusing reigns of Ferdinand VII and Isabel II, particularly to the latter, on which Artola has not previously written. At the level of political history his analysis of the issues in the Carlist War and the conflict between the *Moderados* and *Progresistas*—seen as a struggle over the organization of the state and the means of political control—are excellent. Artola also provides his own interpretation of economic developments. His discussion of the revolution in transportation is especially good. Against Jordi Nadal and Gabriel Tortella he asserts that Spanish legislation did little to retard economic growth; against Vicens Vives and Nicolás Sánchez Albornoz he argues that the Revolution of 1868 was not produced by an economic crisis; and countering common opinion he claims that *desamortización* did not alter the rural structure radically or lead to extensive expansion in agriculture. Spain's comparative backwardness, he argues, came out of its physical and social structure and, measured in absolute terms, its progress was great. Throughout, his reasoning is convincing.

Like those of the Langer series the book is built around a theme: the accession of the revolutionary bourgeoisie to power. Even the

chapter on culture sees it as a bourgeois product. (Culture is broadly conceived and includes the new urban life, travel, and summer vacations.) It is a puzzling theme, for although Artola believes in the existence of a bourgeois class he never describes it in detail, and the sketchy lines he provides do not call to mind the usual picture. The most powerful sector of the new ruling class, he says, given the circumstances, was inevitably agricultural and incorporated the old aristocracy. The riddle becomes clear when one realizes that for Artola the bourgeois nature of the period is revealed not by the characteristics of the dominant class, but by its legal, economic, and constitutional system, which was classically liberal. The new regime was bourgeois because it replaced the older society of orders with a "classist society organized on the doctrines of liberty, equality, and property," and opposed freedom of association (for workers) and the welfare state. Unfortunately his emphasis on the term "bourgeois," heavy with implications associated with more commercial and industrialized countries, suggests the presence of a stronger middle class than Spain had and clouds the perceptiveness of his interpretation.

On the whole, however, this is a very good book. Artola has given his collaborators a hard act to follow. If the rest of the series measures up, it will become the standard survey of Spanish history, replacing, or at least complementing, Vicens Vives's *Historia de España y América*.

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CLARA E. LIDA. *Anarquismo y revolución en la España del XIX*. [Madrid:] Siglo Veintiuno de España Editores. 1972. Pp. 334.

JOSEP TERMES. *Anarquismo y sindicalismo en España: La Primera Internacional, 1864-1881*. (Colección Horas de España.) Barcelona: Ediciones Ariel. 1972. Pp. 670. 750 ptas.

Historians have studied the First International in Spain, 1868-74, using almost exclusively either the prism of ideology or that of the international anarchist movement directed by Michael Bakunin. Working independently of one another, Professors Clara Lida of the State

University of New York (Stony Brook) and Josep Termes of the Autonomous University of Barcelona have brought these events into focus as an on-going problem in Spanish history. They analyze the process by which the ideology and tactics of anarchism became the official program of the Spanish Federation, which was founded by vote of the first national workers' congress in 1870. Neither purports to write a comprehensive account. Termes studies the way anarchism was used to mobilize the Catalan industrial labor force. Concerned with anarchism and revolution, Lida examines the Republican revolutionary movement. Both trace the origins of the First International to Spain's political and labor agitation before 1868 and then move forward to the subsequent organization of the federation.

The Spanish Federation was the public organization, but its motive force was the Alianza, a semiclandestine leadership group inspired by Bakunin. Termes's delineation of the Alianza's role, particularly in the clandestine era after 1874, is an important contribution; he has skillfully integrated the material from the Max Nettlau archives published by Renée Lamberet to complement the classical account of Anselmo Lorenzo. But much more work is needed in foreign archives, particularly in Geneva, to show the precise degree and manner that *Aliancistas* collaborated with Bakunin's international brotherhood.

The solidly new aspect of these works is the close analysis of the division within the ranks of Spanish anarchism: on the one hand, the Catalan labor leaders who needed legality in order to be able to organize a mass workers' movement and thus, in turn, to be able to resist factory owners (the dimension analyzed by Termes), on the other, the Andalusian peasantry who adopted terrorist tactics and clandestine action groups because they constituted the most viable weapons for an impoverished agricultural proletariat residing in agro-towns (the concern of Lida).

The way in which officials of the Spanish Federation assimilated and implemented the program of the First International is charted in greatest detail by Termes who has studied the minutes and directives of the federal councils and commissions, the editorials of the Bar-

celona organ (*La Federación*), and the proceedings of the three labor congresses. But he has also been at pains to describe the reactions of lower echelon officials in Barcelona, and those of workers themselves. In this context he analyzes the ideas that reached workers through popular political ballads and broadsheets and includes an appendix of this source material, invaluable for all historians concerned with what workers really wanted and believed. Although he surveys the strikes called by member unions of the federation during these years, Termes himself points to the need for further study of labor conflict.

Reacting against the interpretation of Andalusian peasant insurrections as originating with the anarchism of the First International, or as messianic in inspiration, Lida considers as precedent the armed insurrections of Republican clandestine conspirators, which she argues incorporated the ideas of Utopian socialism. Her evidence indicates these ideas entered Spain through Andalusian (not Catalan) newspapers. But to prove that they were a political force she must show that peasants knew about and reacted to the ideas. Another contribution is the data from diplomatic and police files in Paris and in Washington showing contacts between Andalusian Republicans and European movements for social and political justice. Finally she offers a new interpretation of the Black Hand criminal trials of 1883, as being both an epilogue to the First International and a protest of desperate Andalusian peasants against the Catalan-oriented tactics of the federation's national leadership. Two difficulties are the nature of the evidence on workers' activities (always suspect as worker, police, and politician accuse one another of fabricating evidence for plots and counterplots) and the tendency to describe isolated examples in the light of new evidence rather than to provide a continuous narrative that would link mid-nineteenth-century Republicans with anarchists. But Lida, like Termes, has provided invaluable new documentation and analysis for the events of the First International as the product of socioeconomic and political realities in Spain itself.

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RAYMOND CARR, editor. *The Republic and the Civil War in Spain*. (Problems in Focus Series.) [New York:] St. Martin's Press. 1971. Pp. x, 275. \$10.00.

PIERRE BROUÉ and EMILE TÉMIME. *The Revolution and the Civil War in Spain*. Translated by TONY WHITE. Cambridge, Mass.: MIT Press. 1972. Pp. 590. \$12.50.

The contrast between Raymond Carr's *The Republic and the Civil War in Spain* and Pierre Broué and Emile Témime's *The Revolution and the Civil War in Spain* is summed up in the single word that differentiates each title. The contributors to the Carr volume cover a lot of ground, but most of their articles revolve around questions of responsibility for destroying the Second Republic, while Broué and Témime focus on the Left and Right revolutions and the diplomacy they necessitated.

First published in French in 1961 Broué and Témime's book is, in my opinion, the best single volume on the Spanish Civil War. It is divided into two discreet parts. Broué deals with wartime peasant and proletarian attempts to crush Republican capitalism replacing it with collectivism. Had the revolution within the Civil War been allowed to proceed, he argues, the popular forces would not have been demoralized as they were after May 1937, and the Loyalist Republican government might have defeated the Nationalists. His thesis is intended to refute claims that only Stalinist communism, allied with the agricultural and industrial petty bourgeoisie in the Republican sector, was capable of winning the war.

Partial supporters and extreme critics of this view are represented in Carr. Hugh Thomas attempts to show how varied and, in some cases, successful agrarian communes were. As a passionate defender of the anarchists and the anti-Stalinist POUM, Burnett Bolloten forcefully laments that, in the name of defeating fascism, the Communist party squelched the revolution. Ramón Salas Larrazábal, concerned with questions of legitimacy, claims that the Left's revolution—not the Fascists or Insurgents—destroyed the republic. And Raymond Carr intimates that what little chance the republic had to succeed was reduced by the anarchists' spontaneous revolution.

Emile Témime takes up the issue of the Right revolution carried on by the Nationalists and the problem of diplomacy. Témime was among the first liberal historians to argue that Francisco Franco was not a Fascist but a skilled authoritarian politician, who not only built a victorious army but co-opted the Falange and welded the antagonistic forces of the Right into a new traditionalist state able to provide social services. From quite an opposite point of view Ricardo de la Cierva portrays the Nationalists as a popular movement. He asserts that public donation paid for the Italian planes necessary for Nationalist victory and that 80 per cent of the northern Insurgent army were eager volunteers not conscripts.

Témime, de la Cierva, Carr, and Robert Whealey all devote space to the international ramifications of the war although Carr supports Gabriel Jackson (the leading American Civil War scholar absent from Carr's collection) in stressing that too much attention has been paid to foreign intervention and not enough to domestic questions. Carr refutes the shibboleth that the Spanish Civil War was the first act of World War II; he says its international importance has been overestimated. The others dissent, and Whealey is quite explicit when he writes: "It can be argued that forceful Anglo-French intervention on the side of the Republic in July 1936 might have saved liberalism" (p. 233).

De la Cierva claims that both sides received equal foreign aid. Possibly the most interesting statement in either volume is his assertion that "the unlimited supply of fuel oil and lubricants on credit from oil companies in the southern United States, and the pressure of Catholic opinion there which kept the embargo on arms for the Republic had perhaps as important an effect on the course of the Civil War as other more highly publicised contributions" (p. 206).

Several of Carr's historians think the republic's salvation lay in a middle road not taken because the Left prevented it. Edward Malefakis conjectures that there might have been a centrist political coalition uniting groups from Prieto Socialists to the Gil Robles' Christian Democratic C.E.D.A., which he defends against charges of Fascist tendencies. He is supported by Richard Robinson who ex-

operates the Right from any blame in polarizing the republic "because it was the revolutionary left which brought about the failure of the legalist C.E.D.A. as the party of the right-wing masses" (p. 74). And Stanley Payne implies that Manuel Azaña and the Republicans were responsible for their own doom because they collaborated with the Socialists during the republic and attacked the army.

Needless to say Franco's army, the militias, and foreign armed forces receive a great deal of attention in the pieces by Témime, Payne, Larrazábal, Whealey, and de la Cierva. Taken together—although they disagree on many points—they show that the army and the Church were the victors on the Nationalist side. Unfortunately the political activities of the Church are considered only in passing in either volume. But old platitudes are put to rest. Both books succeed in showing that the complexity of political groups on the Right matched the more widely recognized divisions on the Left.

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H. V. LIVERMORE. *Portugal: A Short History*. (Short Histories of Europe, 3.) [Edinburgh:] Edinburgh University Press; distrib. by Aldine Publishing Company, Chicago. 1973. Pp. 213. 4 maps. \$9.00.

RICHARD HERR. *Spain*. (The Modern Nations in Historical Perspective.) Englewood Cliffs, N.J.: Prentice-Hall. 1971. Pp. x, 306. \$7.95.

STANLEY G. PAYNE. *A History of Spain and Portugal*. In two volumes. [Madison:] University of Wisconsin Press. 1973. Pp. xvi, 349; xiii, 351-712. \$10.00 each.

It is perhaps not too homely to compare the problem of writing a national history with those of packing for a long trip. Should the author try to take nearly everything, somehow compressing it into the bulging suitcase, or should he cut away the unnecessary parts of his historical garments, covering only the essentials? Perhaps it would be better to pick only the relevant items, and with some daring combination of tuxedo, bathing suit, and sheepskin coat brave all weathers and conditions.

The authors of these three books have

packed their baggage in different ways, and each of their books emerges as a kind of design compromise between narrative continuity, topical clarity, and conceptual insight. Livermore chooses to organize his book around narrative, considering it his task to "narrate the main events of political history with some account of social organization." Moreover he makes this political history complete and deals with Portuguese history from the remotest times to the end of the Salazar regime. Since Livermore has little elbow room (197 pages) the result is a densely packed political narrative interlarded with brief sections on economic and social matters. He gives about one-third of his space to the period up to 1385. The time from the advent of the house of Avis to the rise of the Braganza kings gets about 25 per cent of the pages. The balance of the book brings us up to 1964, with heavy emphasis on the nineteenth century. The focus of the narrative is naturally on metropolitan Portugal and particularly Lisbon. Brazil and the Far East get attention only in passing as their histories bear on that of the Peninsula.

Almost every sentence Livermore writes has one or more items of information to convey. The author's prose is clear if uninspired, and, if the reader is moved by his own interest in Portuguese history, he will find a selection of interesting data. But reader and author both have to pay a price for the decision to organize the book around a political narrative. To my mind Livermore's book is insufficiently conceptualized. He is reluctant to subject his facts to the control of ideas or to make general propositions. The result is not only difficult reading but frequent uncertainty about what the author himself thinks about the facts. Even when he brings together various themes, such as economic revival, Brazilian trade, and intellectual innovation during the ministry of Pombal, somehow the interaction of various factors (the sense of process) does not emerge. Nor is there anything daring or innovative about Livermore's arrangement of the data. The dust jacket of the book provides a capsule review: "[This] book may not modify prevailing views but it will certainly better inform them." In short, then, Livermore's work is a brief factual summary, essentially correct and remarkable for its completeness within the space allowed,

but eschewing attempts at profound penetration or interpretation.

In contrast to Livermore, Herr's *Spain* is highly selective and conceptualized. Herr commences *in medias res* with the Franco government's execution of Julian Grimau, an alleged war criminal and Communist subversive, in 1963. From this he moves to a *tour d'horizon* of the state of Spain twenty-five years after the end of the Civil War. Only then does he move back to the historical roots of Spain. Daringly the author brings us from the Romans to the eighteenth century in just twenty-two pages. He does this by discussing the philosophical interpretations of Spanish history advanced by Unamuno, Ortega, Americo Castro, Sanchez Albornoz, and Vicens Vives. Following their lead he tries to define the Spanish character in terms of historical experience. With the making of Spain disposed of the author turns to the origins of contemporary Spain, which he sees as beginning with the eighteenth century, especially with the reign of Charles III. Herr then proceeds in a more leisurely fashion, using 35 per cent of his pages on the period up to the end of the Primo de Rivera dictatorship. On the period since 1930 Herr becomes more detailed and thorough, devoting over half his book to the Second Republic, the Civil War, and the subsequent Franco regime.

This treatment gives Herr's book cogency and relevance to contemporary concerns as well as a focus that will interest most readers. The author's interpretative device is an interesting elaboration of the "two Spains" thesis. Briefly, he sees the split between enlightened and traditional Spain as paralleled by another gulf between the landless peasantry and the great latifundia. This separation, however, was bridged by the common allegiance of landlord and peasant to Catholic tradition. This transcendent fact rallied the peasantry to traditional politics well into the twentieth century. A third dichotomy is the regional tension between the center of Spain, Castile, and the periphery, most notably Catalonia. This tension has endured to the present.

Like Livermore, Herr has to pay a price for his choice of approach. I cannot suppress a certain unease at the rapidity with which the medieval period is passed over. Many historians think of it as the decisive period of Spanish

history. Are the ideas of Castro, Sanchez Albornoz, etc., suggestive and fascinating as they are, adequate to explain the period and complexity of events? Nevertheless Herr's work is thoughtful, penetrating, and written in a lively style that attracts the attention.

In contrast to Livermore's and Herr's strictly national histories Payne takes the large task of dealing with both Iberian nations. This involves not only a larger study but the additional problems of balance and a kind of implicit contract to make comparisons. To be sure the author has more space with two volumes and 676 pages at his disposal. He, like Herr, seems to regard the eighteenth century as the beginning of the modern period for he divides his work almost equally between the years since 1700 (vol. 2) and all the previous eras of Iberian history. Portugal receives about 16 per cent of the pages, alternating with the more extensive treatment of Spain. The author deals with each country as they passed through parallel stages, and the transition from one to the other is smoothly managed.

Payne conceived his work as both a textbook and an interpretative account and thus committed himself to a compromise, which he nicely achieves, between the requirement of narrative continuity and the demands of explanation. The work is organized essentially around concepts, but with sufficient narrative to make us aware of process without overwhelming us. Moreover the work is careful with facts, and its conclusions are ably drawn. Payne is more cautious in interpretation than Herr. He does, however, advance the highly suggestive idea that Spain should not be compared to Britain, France, and Holland, but rather to the nations of southern and eastern Europe (notably Poland) whose historical experience and social conditions were more similar to Spain's. Some very useful and interesting perspectives flow from these comparisons.

All three works have indexes, useful maps, and charts, while Livermore's has, in addition, some interesting photographs, though it lacks any bibliography or references. On the other hand Herr and Payne have useful and quite complete bibliographies.

On balance the Payne and Herr books must be accounted successful. They carry out their authors' intentions, and they would be useful

as texts or simply as well-done national histories. Herr is perhaps more daring in interpretation, but Payne's judiciousness and broad coverage are attractive. Livermore's work has to be considered as less successful, largely on account of its old-fashioned focus on political narrative and the author's reluctance to conceptualize.

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C. B. WELS, editor. *Bescheiden betreffende de buitenlandse politiek van Nederland* [Documents on Foreign Policy of the Netherlands]. First period, 1848-1870. Volume 1, 1848. (Rijks geschiedkundige publicatiën. Major Series, 139.) The Hague: Martinus Nijhoff. 1972. Pp. xxxi, 714.

This massive collection of documents is part of a government-sponsored project begun two decades ago and now nearing completion, the publication of the archives of the Ministry of Foreign Affairs from 1848 through 1919. As is the case with previously issued volumes this one is ably edited (Wels is a specialist in mid-nineteenth-century Dutch foreign affairs). It includes notably complete indexes, extensive footnotes, a valuable bibliography, and a fine introductory essay. The bulk of the documents (many of which are in French, the diplomatic language of the day) are the reports coming into the Hague, and the replies to them. Arranged chronologically, they add up to an interesting picture of the "year of revolutions" as it appeared from the vantage point of a small but important state.

For the Netherlands 1848 was the year when William II (a brother-in-law of Nicholas I of Russia) accelerated his cautious consideration of Liberal demands (led by J. R. Thorbecke) and granted a constitutional revision that for practical purposes meant a parliamentary regime. The wider events of 1848 doubtless influenced this action. For the Ministry of Foreign Affairs the year was a difficult one. Apart from the ministry's natural aristocratic distaste for the revolutions it soon became clear that specific Dutch interests were involved. Both the Hague and Brussels feared an adventurous French foreign policy, perhaps involving French troops in support of Belgian republicans. In the face of this possibility Dutch-Belgian an-

tagonisms (the treaties finalizing the Belgian separation were only a decade earlier) softened considerably. Of even more direct concern was the problem of Limburg: as a result of the 1839 treaties, it was, curiously, both a Dutch province and a member of the Germanic Confederation, thus owing a dual allegiance. Its representatives to the Frankfort Assembly supported the separation of Limburg from the Netherlands. Hence events in Frankfort and Berlin became a major concern as the year wore on.

Dispatches from Vienna, St. Petersburg, London, Madrid, and elsewhere round out the collection and include passing references to hundreds of players in the historical drama, ranging from Metternich to Lola Montes to Lewis Cass (Zachery Taylor was the preferred presidential candidate). In summary, these documents will interest not only historians of the Netherlands, but also those concerned with 1848, especially those dealing with the Frankfort Assembly.

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GUNILLA INGMAR. *Monopol på nyheter: Ekonomiska och politiska aspekter på svenska och internationella nyhetsbyråers verksamhet 1870-1919* [Monopoly in News: The Economic and Political Aspects of the Operation of Swedish and International News Agencies 1870-1919]. (Scandinavian University Books. Studia Historica Upsaliensia, 52.) [Stockholm:] Esselte Studium. 1973. Pp. xiv, 240.

International agreements between European press associations sought to reduce competition and assure profits. The first pact of 1859 proved too limited and a second of 1870 among Reuters, Havas, and Wolff (British, French, and German) framed a cartel arrangement that lasted through World War I. Even with renewals the arrangement withstood political pressures from governments and adverse national opinion, proving that profits counted for more than national interest. The press associations divided the world into spheres of operation, just as the political powers carved out zones of influence.

The Swedish press association Telegrambyrå, a private venture with Foreign Office support, received a portion of its international news via

Ritzau in Copenhagen. Because Sweden and other northern countries fell within Wolff's sphere, during World War I another press association, Nordiska Presscentralen, was created to disperse Allied news. Ingmar's description of the conflict over control of Telegrambyrå and its competitors demonstrates the subordination of the Swedish press to both the Germans and monopoly control by the major news services. The full implications of the major news bureaus influence on governments and international relations is, however, somewhat lost, both through neglect of works such as those of Oren Hale and E. Malcolm Carroll and through concentration on financial aspects and intrigues by individuals trying to maintain their grasp. This latter point is especially damaging to the detailed study of Telegrambyrå, where a content analysis of news would easily have proven the point of the thesis. On the whole it appears that governments influence news gathering and distribution negligibly, as when Wolff refused to do the German government's bidding or where the same government permitted use of Reuters and Havas services in World War I.

Although too brief to permit a full detailed study of the international press associations or Swedish Telegrambyrå this effort contributes knowledge on the subject. Its summaries (often with unnecessary duplication), publication of texts of agreements, and bibliography on a specialized topic are a contribution worthy of note for the historian interested in the press.

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SEPPO MYLLYNIEMI. *Die Neuordnung der baltischen Länder, 1941-1944: Zum nationalsozialistischen Inhalt der deutschen Besatzungspolitik.* (Dissertationes Historicae 2; Historiallisia Tutkimuksia 90.) Helsinki: Suomen Historiallinen Seura. 1973. Pp. 308.

At a time when students of Nazi Germany are increasingly emphasizing the major personalities at the center of power a young Finnish historian Seppo Myllyniemi, has given us an institutionally-oriented examination of rule at the periphery—German wartime occupation policy in Estonia, Latvia, and Lithuania. Based on a wide variety of archival sources Myllyniemi's detailed work largely substantiates

the major themes of the Baltic chapter of Alexander Dallin's monumental *German Rule in Russia*.

The Nazis' long-range goal of incorporating the Baltic area into the Reich clashed with and ultimately superseded the immediate task of winning the war in the East. Hence, after lengthy debate the Germans refused to grant the minimum popular demand for semi-independent protectorate status for the three former states, made only token restitution of private property earlier nationalized by the Russians, and waited until military defeat was staring them in the face before authorizing sizable native armed forces in Latvia and Estonia.

This impractical rigidity was grounded in Nazi racial theory. The Baltic area fell into a middle category in Hitler's New Europe roughly parallel to Bohemia and Moravia: a site for future German colonization where the indigenous population contained large segments that were potentially Germanizable (*eindeutschungsfähig*). Persistent attempts to convince the lucky ones of their Nordicness, however, usually ran afoul of national pride, while severe colonial exploitation eroded the initial enthusiasm for the Germans as liberators from Soviet oppression.

Haphazard policy execution compounded the problems caused by contradictory goals. Irreverent Berliners quickly dubbed Alfred Rosenberg's East Ministry the "*Chaostministerium*," and the *Reichskommissar* for the Baltic, Hinrich Lohse, presented a particularly vivid picture of fanatical ineptitude. Moreover, Rosenberg's losing battles with Himmler, Göring, and Koch further confused German Baltic policy; the helpless ideologue was still concocting administrative reforms when the Red Army was at the doorstep. A careful reading of this book will surely disabuse one of any lingering belief in the alleged efficiency of dictatorships.

The author is especially effective in tracing several tragicomic ideas of little men in the party bureaucracy. For example, the Estonians were judged to be racially superior to the Latvians, who in turn were supposedly superior to the Lithuanians. Unfortunately the Estonian language, a Finno-Ugric tongue, did not correspond to the Nazi racial-linguistic schema,

so officials were cautioned against bringing up this touchy subject.

A weakness of this book is the failure to discuss thoroughly the effect of German policy on the daily lives of the Baltic natives. Though Myllyniemi's dry descriptions of bureaucratic affairs often make for difficult reading his study nonetheless is a valuable contribution to an understanding of Nazi occupation policy.

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CARL HAASE. *Ernst Brandes, 1758–1810*. Volume 1. (Veröffentlichungen der Historischen Kommission für Niedersachsen und Bremen, 32. Niedersächsische Biographien, 4.) Hildesheim: August Lax Verlagsbuchhandlung, 1973. Pp. xvi, 443. DM 48.

This volume by Carl Haase is obviously a labor of love. Based on an exhaustive study of all of Brandes's published writings and reviews as well as on unpublished archival material in the Westphalian archives, this book discusses the total literary achievement of this interesting exponent of enlightened conservatism during his formative and early adult years. After discussing Brandes's early years within the framework of local Westphalian history, Haase summarizes Brandes's writings through paraphrase and comment and relates them topically to the issues and debates current in Germany at the end of the *ancien régime* and during the first years of the French Revolution.

The first hundred pages of this often tedious study present Brandes's biography until 1791 against the backdrop of institutions and personalities that have either been neglected altogether or have received cursory mention in previous studies of the period. I found this part of Haase's book the most interesting, as it refers to such fascinating tidbits as the association of Ernst Brandes's father, Georg, with Moses Mendelssohn, the friendship of the Brandes family with Heine and Forester, the academic politics in Göttingen, and the bureaucracy and social structure in Hannover during the late eighteenth century. Based on an intimate knowledge of many of the printed and unpublished sources, this part of the study suggests among other things a "republic of letters" that crossed political lines. It is unfortunate that Haase leaves too much to the

imagination here, a failing conspicuously absent from the remainder of the book.

The rest of this study is concerned with topically presenting Brandes's views on a variety of issues ranging from the theater, the English constitution, secret societies, women, the relationship between the nobility and the middle classes, the role of the lower classes, the science of history, social life in Hannover, and contemporary morals to the French Revolution. It is the last of these that has received the most attention from scholars—rightfully so, in my opinion, as Brandes was not only one of the first German writers to approach the study of this momentous event critically but also followed the course of events with attention and judicious insight. Haase's own concluding chapter on Brandes and the French Revolution is interesting, if not fascinating. Unfortunately many of the chapters that precede seem to be there only to support Haase's contention that Brandes remained consistent throughout his career in his politics, his Anglomania and in his prudent avoidance of controversy with such politically dangerous arch-conservatives as Johann Zimmermann. Haase consequently drags his reader through a lengthy presentation of Brandes's views of the prerevolutionary period where any humor that might offset the banality of Brandes's arguments is lamentably absent. Indeed, if it were only for the chapters on the theater, history, women, and so on, one might well wonder whether it would not have been kinder to disregard the plea of the late Klaus Epstein for a study of Brandes's achievement and leave Brandes in the partial obscurity that his thought on these subjects merited. This impression is unfortunately exacerbated by Haase's frequent attempt to present Brandes's views as profound and even relevant to our times.

The bibliography is excellent and, unlike the bulk of the book, valuable for any serious student of late eighteenth-century German intellectual history.

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WILHELM LIEBKNECHT. *Briefwechsel mit deutschen Sozialdemokraten*. Volume 1, 1862–1878. Edited, with an introduction, by GEORG ECKERT.

(Quellen und Untersuchungen zur Geschichte der deutschen und österreichischen Arbeiterbewegung. New Series, number 4.) Assen: Van Gorcum & Comp. 1973. Pp. lii, 908. 116 gls.

This superbly edited collection is only the first installment of a projected multivolume edition of Wilhelm Liebknecht's correspondence with socialists and other radicals from 1862 to his death in 1900. Liebknecht played a key role in the Social Democratic movement, not only within Germany but also internationally, and one naturally expects that his extensive exchange of letters will be an exceptionally valuable source for the history of European socialism. The expectation, however, is not completely fulfilled in this first volume. One disappointment is that only 103 of the 534 letters in the main body of the text are by Liebknecht. As a result it is seldom possible to get a sense of thematic continuity through a sustained exchange between Liebknecht and another person. The lacunae are not the fault of the editor, Georg Eckert, who carried on a thorough search for missing Liebknecht letters and had the cooperation not only of the International Institute for Social History in Amsterdam, which is sponsoring the publication, but also of the Institutes of Marxism-Leninism in East Berlin and Moscow. Eckert has been able to compensate to some extent for the gaps by the use of related documents and explanatory footnotes. But the nature of the available material made it inevitable that the volume would have a certain diffuse and fragmented character.

These letters have something to say on nearly every issue related to the early socialist movement, but it is also amply clear that Liebknecht's correspondents were concerned predominantly with practical matters of organization and day-to-day tactics. Broad questions of policy or socialist theory come up only occasionally. The fact is that Liebknecht was far less immersed in theoretical problems than his pamphlets and speeches often lead people to assume. This point was already evident in his correspondence with Marx and Engels, also edited by Georg Eckert (1963). The practical orientation, however, does not diminish the value of this documentation for historical purposes. On the contrary, for these letters impart a mood of personal immediacy and contain a

great wealth of detail on a variety of topics, including conflicts between Lassalleans and Eisenachers, problems of early socialist journalism, relations with the First International, tensions within the Austrian labor movement, socialist attitudes on the Franco-Prussian War, the trial of Liebknecht, Bebel, and Hepner for high treason in 1872, and the merger of the two socialist parties in 1875. One also finds here a rewarding source for information on a number of lesser-known personalities; for example, the letters by Robert Schweichel, the nearly forgotten author of socialist belles-lettres, give some interesting hints on how early socialists approached the arts. When this project is completed—there are at least three volumes to come—it will certainly be one of the major sources for the history of German and European socialism.

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HENRY ASHBY TURNER, JR. *Faschismus und Kapitalismus in Deutschland: Studien zum Verhältnis zwischen Nationalsozialismus und Wirtschaft*. Göttingen: Vandenhoeck & Ruprecht. 1972. Pp. 185. DM 18.

This little volume contains the German translation of Professor Turner's recent articles, all but one of which originally appeared in American historical journals, including the *AHR*. It is most appropriate that the author's conclusions become known to a larger audience, for they explode some myths.

While the essays were designed for separate publication and therefore do not have a consistent thesis that ties them together as a book, the reader detects as a general theme the relations between big business and right-wing politics in Weimar Germany; the last essay analyzes fascism as an antimodern phenomenon. More specifically, the first four articles attempt to answer the question, "Did German big business support Adolf Hitler's climb to power?" After all, such prominent tycoons as Emil Kirdorf and Fritz Thyssen were closely linked with Hitler's movement several years before he seized power. Turner convincingly demonstrates that Hitler's party received very substantial financial support from business, but only after, not before, Hitler became chancellor. A secret brochure by Hitler,

which is contained in the second essay of this volume, shows that he had succeeded in gaining the support of Kirdorf as early as 1927. As the third essay in this collection reveals, however, the relations between the "Bismarck of the coal industry" and the Nazi party were characterized by Kirdorf's admiration for Hitler but only a short-lived membership in the party, from which he resigned in a huff in 1928. Both Kirdorf and Thyssen, whose ghost-written *I Paid Hitler* (1941) is critically analyzed for factual accuracy in the fourth essay, lent, for the most part, just their names and reputations to the Nazi movement before 1933 rather than the alleged vast sums of money mentioned by numerous historical works. Similarly, the *Ruhrlade*, the secret cabinet of the leaders of the largest coal and iron firms who regularly met after 1928 to deal with problems of common concern, made only very modest monetary contributions to the Nazis before 1933. The analysis of this subject is followed by the concluding essay that characterizes National Socialism as "a utopian form of anti-modernism" and raises but does not answer the question as to whether Italian fascism and other fascisms can be similarly perceived.

These essays give answers to several of the problems raised, but they should also prove a strong stimulus for further research. Since the author made only minor editorial changes in the essays, one wonders if a substantive introduction would not have enhanced the unity and sharpened the perspective of this well-translated volume.

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WERNER MASER. *Hitler: Legend, Myth & Reality*. Translated from the German by PETER and BETTY ROSS. New York: Harper and Row. 1973. Pp. viii, 433. \$12.50.

Over the past twenty-odd years Werner Maser has collected more bits and pieces of "Hitleriana" than anyone alive today. This clearly makes him the world's leading "Hitler buff," though not necessarily "Germany's foremost authority on Adolf Hitler," as his American publishers claim. He has used this information, some significant, some irrelevant, in a number

of repetitious books, of which this one is the first to appear in English. It differs in many parts from the German original, *Adolf Hitler: Legende, Mythos, Wirklichkeit* (1971), which is more detailed and about a hundred pages longer. There is no indication who made these changes. They do not seriously affect the general tenor of the book, however, and they make it more readable.

Except for the opening chapters dealing with Hitler's early life and the last one on his activities in World War II, Maser's book is not so much a biography as a series of essays on various aspects of Hitler's career and personality. Their purpose is to correct whatever "legends" have grown up around the Führer. It should be noted that some of these legends have already been challenged by earlier writers and others have never been taken very seriously.

On Hitler's family background Maser discounts the rumor that his grandfather was Jewish, and he adduces further evidence of inbreeding among his forebears. Hitler's childhood and youth were far less dismal than the Führer describes in *Mein Kampf*. As artist and architect, Hitler, according to Maser, had far more talent than he is usually credited with. His record as a soldier in World War I was "comradely, level-headed and uncommonly brave" (p. 83). A long discussion of Hitler's intellectual background emphasizes the influence of some hitherto neglected figures, notably the biologist Wilhelm Bölsche, and describes Hitler as a voracious reader with a "sceptical, imaginative, speculative and ceaselessly fertile" mind (p. 191). Hitler's sex life, Maser states, was normal, and he denies that Hitler had syphilis. Much attention is given to Hitler's illnesses. New evidence confirms, though fails to explain satisfactorily, Hitler's rapid physical decline, starting in 1942. (The subject of the Führer's health deserves further study by more qualified specialists.) It also provides the theme for the last chapter dealing with Hitler's role as military leader. The one point that has caused most debate in Germany is Maser's assertion that the remains on which the Russians based their famous autopsy report—Lev Bezymenski, *The Death of Adolf Hitler* (1968)—were not Hitler's.

Maser's book, as these few samples show, covers a lot of interesting ground. The reader,

however, who hopes to gain from it a better understanding of the Hitler phenomenon comes away disappointed. There is no attempt made to fit the new evidence into the traditional Hitler image. Without some such synthesis a reader unfamiliar with the subject might gain the impression that Hitler "wasn't really so bad," that he was a normal person of wide-ranging artistic and intellectual gifts who suffered from innumerable illnesses for which he was given the wrong medications, which then made him do horrible things. To create such an impression, of course, was far from the author's mind. But one wonders if, in concentrating on a number of minor "legends," Maser is not, unintentionally, helping to restore the major "myth" that haunted Germany for twelve terrible years.

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ANTHONY NICHOLLS and ERICH MATTHIAS, editors. *German Democracy and the Triumph of Hitler: Essays in Recent German History*. (St. Antony's College, Oxford, Publications, number 3.) New York: St. Martin's Press. 1971. Pp. 271. \$12.95.

HORST VON MALTITZ. *The Evolution of Hitler's Germany: The Ideology, the Personality, the Movement*. New York: McGraw-Hill Book Company. 1973. Pp. xiv, 479. \$12.95.

The essays assembled by Nicholls and Matthias in *German Democracy and the Triumph of Hitler* are all thoughtful and suggestive. Indeed some are cogent and compelling. These papers, largely the work of young historians, were originally presented in seminars at St. Antony's College, Oxford, and the University of Mannheim. Although diverse and wide-ranging in topic and technique, most of the essays portray the political pathology of Weimar Germany. Several authors convincingly restate or reinforce familiar theses concerning the syndromes of unimaginative German liberalism and socialism. Other essayists suggest new sources, signs, and symptoms of early Nazism. In "Hitler and the Bavarian Background to National Socialism" Anthony Nicholls finds Hitler's Bavarian experience so "crucial and seminal" that he conjectures that "had Hitler tried to establish himself in any other German capital in 1919 it is almost inconceivable that he could have

achieved the same results" (p. 101). Such tantalizing, if unprovable, speculation is fortunately accompanied by fascinating and closely reasoned arguments about the Bavarian roots of Hitler's foreign and internal policy plans.

This anthology also includes essays on the impact of the Versailles Treaty and of Article 48 on Weimar developments, as well as studies of Hitler's ideas about the Western powers and of the legacy of 1918 for National Socialism. Especially well documented is Robin Lenman's article on "Julius Streicher and the Origins of the NSDAP in Nuremberg, 1918-1923." Careful archival research shapes this detailed narrative of internal Nazi party strife on the eve of the Munich putsch. Jill McIntyre's stimulating essay, which also relies on extensive primary sources, separates Nazi rhetoric from the reality of the German woman's participation in professional life during the 1930s. She concludes that despite the Third Reich's initial ideological antagonism toward professional women, "the net result of the decade was one favourable to women in the professional occupations" (pp. 212-13).

One comes away from this book refreshed by the various brief explorations of novel approaches and new evidence, along with the clarification of prevailing theses. Yet one regrets the lack of a substantial, interpretive introductory essay that would attempt to assess the larger meanings and broader implications of the diverse articles in this volume. Despite the disparity in essay topics, common themes and conflicting or overlapping interpretations might have been compared, analyzed, or synthesized.

A Herculean but disappointing attempt at synthesis characterizes Horst von Maltitz's *The Evolution of Hitler's Germany*. The book promises much but produces little that scholars will find new or fresh. Intended for a popular audience, two-thirds of this massive volume examines Nazi ideology. Except for original research in Hitler's writings, the author essentially summarizes or excerpts secondary sources. Maltitz, a lawyer by profession, has read widely and buttresses his arguments with frequent references to leading historians and psychologists. The main merits of this work lie in the author's clear, journalistic writing style (supplemented by trenchant

quotes) and his willingness to take seriously the significance and sources of Nazi ideology.

Disconcerting, however, is Maltitz's disproportionate stress on "Jewish destructiveness" in the Weimar era: "The intellectual German Jews were almost exclusively critics rather than builders, and this at a time when the tender flower of the Weimar Republic desperately needed whatever constructive care and help it could get from any source" (p. 152). Maltitz concludes that "paradoxically it is the absence of a liberal Jewish intelligentsia which may contribute to the stability of the German democracy today, in contrast to the Weimar Republic" (p. 457). Other shortcomings of the book include its uneven, episodic, and disjointed organization and character. While claiming that Nazi ideology "centered around" *Lebensraum* rather than anti-Semitism (pp. 180-81), Maltitz devotes ten pages to the former and over one hundred pages to the latter. The limitations of the author's impressionistic intellectual history are evident in his chapters on ideology, which are too often superficial summaries rather than probing analyses.

Moreover, Maltitz fails to place Nazi ideology in the larger European framework of fascism. Potentially misleading is his claim that "the great bulk of the ideology was uniquely German and had no counterpart elsewhere" (p. 271). Unfortunately the author totally overlooks Ernst Nolte's seminal study, *Three Faces of Fascism* (1963, 1965). These criticisms should not, however, detract from the often perceptive discussion of Hitler's personality, including his "religiousness." Occasionally Maltitz's suggestions and speculations invite interest in areas open to continuing scholarly treatment.

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LUDWIG VOLK. *Das Reichskonkordat vom 20. Juli 1933: Von den Ansätzen in der Weimarer Republik bis zur Ratifizierung am 10. September 1933.* (Veröffentlichungen der Kommission für Zeitgeschichte bei der Katholischen Akademie in Bayern. Series B: Forschung, volume 5.) Mainz: Mathias-Grünwald Verlag. 1972. Pp. xxvii, 266. DM 48.

In July 1933 Hitler's Germany signed a concordat with the Vatican that gave prestige to

the new Nazi regime and offered concessions to the Holy See that no Weimar government had given. The reasons of both parties entering into this agreement have been debated since its inception. Volk has now presented the background of this treaty based upon documentation drawn from both Church and state archives in order to show the motives of the participants and how they interacted upon one another. The volume is another in the series published by the Kommission für Zeitgeschichte of the Catholic Academy in Bavaria, which has already published documentary collections about the concordat.

After summarizing the historical debate about the concordat, Volk describes the attempts to reach a Church-state agreement during the Weimar era and the negotiations for the Bavarian and Prussian concordats, and he explains the importance of the school question in German politics for German-Vatican relations. Volk concludes that technical obstacles and political opposition to a German-Vatican treaty during the Weimar period made the possibility of its realization very unfavorable during that time.

In 1933 the Nazis made the first suggestion of once more taking up the question of a concordat. Volk clearly shows the role of participants such as Franz von Papen, Ludwig Kaas, and Eugenio Pacelli in the various negotiating stages as well as the attitude and influence of the German hierarchy upon both parties. Taking the reader through all the negotiations up to and including the signing and ratification of the treaty, the author contends that the Nazis' primary goal in initiating the talks was to induce the Vatican to depoliticize the clergy, but when the Center party dissolved itself in 1933, which the Nazis had not expected at that moment, only then did the secondary motive for a concordat, that of gaining prestige for the regime, gain in importance. Volk says that the Vatican, on the other hand, fully appreciated the evil in nazism but had no other choice than to sign the concordat to preserve some safeguards for their schools and social organizations that the Nazis were sure to abolish. With an internationally legal framework in which to work, Rome hoped to fight back, since Hitler's guarantees within Germany had proved doubtful at best.

Volk's work is thorough, based on detailed research, with many quotations in the footnotes, including some from the all but inaccessible Vatican Archives of the period. One final point might be added. Despite Volk's description of the Vatican's intentions, one still might like a further discussion about whether the Vatican, or any other power for that matter, saw the complete moral and diplomatic consequences of signing a treaty with Hitler.

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WILLIAM CARR. *Arms, Autarky and Aggression: A Study in German Foreign Policy, 1933-1939*. (Foundations of Modern History Series) New York: W. W. Norton and Company. 1973. Pp. vi, 136. \$7.00.

Studies of Nazi foreign policy seem to follow clearly recognizable patterns. Until the mid-sixties they tended to focus either on Germany's relations with the other major European powers or on the narrative account of the main and familiar way stations on the road to war, from the Anglo-German Naval Agreement through Hitler's march into Prague. More recently the internal rivalries between National Socialist party organizations and the *Auswärtiges Amt* have received monographic attention, as has the role of Nazi ideology in the conduct of Hitler's foreign policy. Carr, the author of two previous books on nineteenth-century German history, is not only aware of these trends but has based his slim volume rather heavily on secondary works that exemplify them. For his discussion of Hitler's ideology Carr relies very much on the German edition of Eberhard Jäckel's *Hitlers Weltanschauung* (1969), while Carr's description of the diplomatic developments between the wars seems to owe much to Gerhard L. Weinberg's *The Foreign Policy of Hitler's Germany* (1970). This procedure might be quite legitimate in itself, considering that the title of the study carries the promise of examining Hitler's foreign policy in the light of Nazi economic policy and rearmament. That promise, however, remains largely unfulfilled.

Carr's basic argument and thesis is as logical and simple as it sounds convincing. Given Hitler's ideology or *Weltanschauung*, with its twin goals of racial purity and territorial expansion and his belief that war was not only in-

evitable but salutary, it does not seem surprising that he should have deduced the need for German rearmament from his ideological premises. Since even the Führer could not overlook the fact that in the twentieth century an effective army needs an industrial and economic base of considerable strength and independence from foreign interference, the connection between ideology, autarky, and rearmament for aggression seems to establish itself almost self-evidently. Unfortunately the author never presents an account that demonstrates in convincing historical detail his logically cogent thesis, nor does he achieve an integration of Hitler's foreign policy with his economic and military policies.

In part this is because of Carr's attempt to juggle too many balls at once and to do so in a space too narrowly confined. Thus he not only tries to integrate narratively the stories of rearmament and the drive for economic autarky with the development of foreign policy; he also endeavors to overcome what he sees as the traditional "primacy of foreign policy" (p. 1) approach by emphasizing the interplay and mutual dependence of external or foreign policy with internal or military and economic factors. The weakness of this book also stems in part from the author's indecision about his audience. The bulk of the material recounted is so well known that its presentation can profit only the beginning student. However, the assumptions and conclusions of at least some of the secondary materials used in this study are by no means universally accepted; their presentation and discussion would thus seem to be most fruitfully directed at the specialist. In sum, this brief book falls between too many stools to live up to its title or to the promise and potential of its thesis. It is hoped that another attempt will be made to demonstrate the validity of the core propositions of this work, but that it be done on the basis of primary sources and within a scope adequate to the magnitude of the task.

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ERNA LESKY and ADAM WANDRUSZKA, editors.
Gerard van Swieten und seine Zeit: Internationales Symposium veranstaltet von der Universität Wien im Institut für Geschichte der

Medizin, 8.-10. Mai 1972. (Studien zur Geschichte der Universität Wien, number 8.) Vienna: Verlag Hermann Böhlau Nachf. 1973. Pp. 194. DM 54.

This volume originated from a symposium held to commemorate the two hundredth anniversary of Van Swieten's death. The symposium focused on his significance for the cultural and scientific life of the Habsburg monarchy during the Enlightenment. Of the eleven papers, four deal with "his times" more than with Van Swieten himself and a fifth concerns his son, Gottfried. For publication the editors have added a significant section of documents and an extensive bibliography.

As Erna Lesky indicates, we lack a thorough study of Van Swieten, in part because we lack sources. This collection illustrates the painstaking process of reconstruction that consequently confronts historians. It is not surprising that we do not obtain a coherent accounting of his life and work, but a consistent view of Van Swieten as a reformer emerges. Lesky provides the critical perspective: that everything Van Swieten did in his public life had roots in his deep sense of social accountability, which in turn derived from the social sense of the Leiden Catholic community. Van Swieten was forty-five when he moved to Vienna, and he did not readily leave his homeland; he was not then rejecting his past. G. A. Lindeboom aptly remarks that the Leiden period is the andante to the symphony of his career. Our knowledge of those early years is still sparse, although his correspondence with Antonio Nunes Ribeiro Sanches gives us crucial insights into the beginning of Van Swieten's career in Vienna.

Van Swieten's medical reforms (Christian Probst), his educational reforms (Kalman Benda), and his views on censorship (Grete Klingenstein) reflect the essential unity of his career. To be sure, Lesky and Klingenstein stress that Van Swieten was no Jansenist, while Lindeboom, more judiciously, admits some Jansenist influence on his youth. And Klingenstein sees Van Swieten more distinctly as a man of the Enlightenment than as the Catholic Leidener. But the images remain congruent: as reformer, Van Swieten had a vision of the world even if the definition of that vision and its relationship to the particulars of his own life continue to elude us.

Ultimately this volume serves best to underline and clarify aspects of our understanding of Van Swieten rather than to provide us with striking new insights or a balanced introduction to his life and times.

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ADRIAN LYTTTELTON. *The Seizure of Power: Fascism in Italy, 1919-1929*. New York: Charles Scribner's Sons. 1973. Pp. 544. \$17.50.

NINO VALERI. *Tradizione liberale e fascismo*. (Quaderni di storia.) Florence: Felice Le Monnier. 1972. Pp. viii, 228. L. 2,800.

Lyttelton's impressive book invites comparison with Karl Dietrich Bracher's *Die nationalsozialistische Machtergreifung* (1962) and the first half of Bracher's *The German Dictatorship* (1970), which summarizes his views about the Nazi seizure and consolidation of power. According to Bracher "the National Socialist take-over and the organization of the Third Reich essentially was the social revolution of the professionally handicapped or unsuccessful petty bourgeois with military training or tendencies, who harbored great resentments against the establishments of both the Left and the Right. Hitler was their perfect embodiment" (*The German Dictatorship*, p. 276). Lyttelton comes to essentially the same kind of conclusion concerning Fascist Italy and Mussolini. But whereas Bracher tries to explain Hitler, Lyttelton's frequent labeling of Mussolini's behavior as "pragmatism" or "realism" begs the question of the Duce's ideas and motivations. On the other hand Lyttelton is more thorough than Bracher in analyzing those forces that the Fascist dictators had to placate or eliminate, particularly big business, the other political parties, and nonconformists within their own party. The Nazi and Fascist regimes created police states, but, as Lyttelton rightly notes, "in Germany, the police administration fell into the hands of an ideologically inspired elite, originating in the ranks of the party. In Italy, it remained under the control of trained bureaucrats. . . . The police, under Bocchini's efficient management, largely absorbed the repressive functions of the party and the squads" (p. 297).

The main differences between Bracher's and Lyttelton's accounts reflect the slower and

more halting pace of the seizure of power in Italy. Whereas in Germany the party dissidents were crushed and the rest of society was brought under Nazi control within a year and a half after Hitler became head of the government, it took Mussolini six and a half years to consolidate his regime. Nevertheless the descriptions of the regime's economic policies and the fate of the Fascist unions in Lyttelton's chapter on "The Fascist Economy" are not convincingly related to the political struggle. The same is true for most of the material in the chapter on "Ideology and Culture." The chapter on "Propaganda and Education," particularly the section on control of the press, is certainly important for Lyttelton's main theme, but the concluding chapter, "The Regime," is sketchy on the negotiations leading to the Lateran Accords with the Vatican, which presumably justify the 1929 terminal date of the book.

Despite its occasional *longueurs* and allusiveness Lyttelton's book is the best account in any language of the Fascist seizure of power and a major contribution to historical scholarship by any standards. The section on the crisis in the party and the Militia, which followed the Matteotti crisis, is excellent. And Lyttelton gives the most convincing explanation available for the survival of Roberto Farinacci (in contrast to Ernst Röhm in Nazi Germany) as a gadfly of the regime. He also provides new insights concerning the alliances of rival Fascist factions with or against rival banking groups, especially the Giolittian Banca Commerciale. Although all his facts are well documented some of his speculations are based on partisan secondary sources. But Lyttelton's greatest achievement is the way he weaves together the complex interplay of conflicting political groups, the economic situation, and Mussolini's concern about foreign opinion.

Valeri's book deals with the historiography of fascism. Unlike Costanzo Casucci, Renzo De Felice, and A. William Salomone in their anthologies, Valeri incorporates the main ideas of diverse critics into his own analysis. Starting with H. Stuart Hughes, Federico Chabod, and Christopher Seton-Watson, Valeri reviews a number of works of the 1960s that try to link fascism with elements of Italian national character and behavior. In chapter 3 he discusses the published letters, notebooks, and memoirs

of some of the leading Italian politicians and critics of the period immediately following the First World War. Chapters 4 and 5 are devoted to various interpretations of the European character of fascism and the totalitarian ingredient in contemporary civilization. In chapter 6 Valeri discusses polemics and critical works about the last years of Italian fascism. He also reviews certain popular books—Luigi Barzini's *The Italians* (1964) and Antonio Pellicani's *Il filo nero* (1968)—which try to give "authoritative" explanations that professional historians are unable to provide concerning the "whys" of fascism for ordinary people who are conditioned to expect instant analysis from the mass media. But unlike De Felice in his *Le interpretazioni del fascismo* (1969) Valeri completely neglects the contributions of political sociologists to our understanding of the nature of fascism.

In chapter 7, "The Problem of Responsibility," Valeri argues that a true understanding of history must be distinguished from polemical interpretations of what happened in the past in terms of present political, practical, or ideological concerns. He willingly accepts the label "bourgeois liberal" for this view and criticizes some of its most eminent older spokesmen, such as Croce and Meinecke, for deviating from it in saying that fascism was caused by gangs of adventurers taking advantage of an extraordinary and temporary crisis situation. Valeri insists on "the objective impossibility of isolating, in that chaotic situation, one fact or group of facts and attitudes and making it the sole or dominant explanation of the tragedy" (p. 220). Like Ernst Nolte, Valeri believes that fascism was a basic phenomenon in European history in the period after 1919.

Valeri's book is the swansong of a liberal historian who has come to believe that conditions rather than men determine the course of history. Valeri is undoubtedly correct in warning us against seeking the one true explanation. But surely the professional historian must try to explain how the conditions that make things happen came about in the first place. Lyttelton, a much younger liberal, does this successfully by emphasizing the role of men as agents of political forces.

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GIORGIO BOCCA. *Palmito Togliatti*. Bari: Editori Laterza. 1973. Pp. 753.

He was cold, cutting, sometimes cruel, a bit of a snob, suspicious of zeal, alert to the imperatives of power and the prerogatives of rank, an authoritarian, a dangerous enemy, and an unreliable friend. His comrades called him "the Torinese professor" and found him hard to like. Celebrated for his double-breasted suits, his shrewdness, and his quiet, courteous, almost pedantic manner he cut a strange figure as the leader of the largest revolutionary party in the West. His mastery of maneuver and his tactical flexibility—what the Italians call in a splendid and untranslatable word "*spreguidicatezza*"—called to mind men of state like Machiavelli, Cavour, or Giolitti rather than idealists like Garibaldi and Mazzini, whose vision of what *might be* often obstructed their ability to see what *was*. Like so many of his countrymen, Togliatti prided himself on his ability to read reality rather than to change it. And yet few men shaped Italian postwar politics more than he did—perhaps only the Agnellis and the Popes.

Giorgio Bocca has told his story on the basis of published sources and a large number of interviews with those who were close to Togliatti at various times: his family, his friends, his lieutenants, his rivals, and his victims. The result is a fascinating book. Bocca knows how to sift the evidence, search out the facts, and shatter the legends created by party hacks and hagiographers without losing sight of Togliatti's considerable achievement. He judges and sometimes he judges harshly, but his judgments have the ring of truth and the virtue of compassion. It is naive, Bocca says, to think that Togliatti was not a convinced Stalinist. Indeed he rose to power in the first place because he was willing to implement Stalinist policies that other Italian Communist leaders rejected as mistaken or absurd. Bocca's chapter on Stalin's Comintern, his best, evokes the sinister atmosphere of the Hotel Lux during the era of the Purges and explains why even the best of men fell prey to the fascination of Stalin's power, demonstrating in sad detail that if Togliatti survived it was because his dominating characteristics were caution rather than courage, prudence rather than the pride that comes from un-

yielding integrity. Nor, Bocca goes on to warn us, should we suppose that Togliatti had undergone a sudden conversion to the values of pluralist democracy when he returned to Italy in 1944 and shocked many of his supporters by calling for a government of national union. He simply acknowledged that given Italy's situation there was no other way to power than through collaboration with other antifascist and sometimes recently fascist elements. Finally, Bocca shows that while it is true that Togliatti had a great respect for humanistic culture and an attitude of condescension toward those who did not possess it he never viewed it as an end in itself, but only as a means for reshaping Italian social reality. When it served his purpose he did not hesitate to bend the truth, suppress an embarrassing fact, or rewrite history. Togliatti's later liberalism was forced upon him by events.

Bocca is quick to speculate when documents are missing or when his informants have omitted important facts. He may have made mistakes and sinned through an excess of demystifying zeal. But whatever the errors of detail, he has written in the spirit of Togliatti himself who advised his comrades when evaluating Stalin to seek to understand the circumstances in which he had operated. "Believe me," he once said, "in great part what he did could not have been done differently." Bocca comes to a similar conclusion about Togliatti and views the PCI as a progressive force in postwar Italian life. Because of it, he argues, Italian culture is livelier, Italian democracy is healthier, and Italian capitalism is more humane. It is hard to disagree.

ROBERT WOHL
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STEPHEN GAZI. *A History of Croatia*. New York: Philosophical Library. 1973. Pp. xv, 362, 6 maps. \$11.95.

This is a simple narrative of the political and military history of Croatia to the outbreak of World War II. A concluding chapter summarizes events since then in ten pages. Another chapter, essentially a catalog of names and titles, sums up Croatian contributions to literature and the arts. It is discouraging to find that

historical writing that is so antiquated in approach and so lame in execution can still find a publisher. Most of this book is merely a chronological recital, often a detailed listing, of dynastic changes, military campaigns, diplomatic alliances, territorial adjustments, and electoral results. A bit of superficial analysis is occasionally given in the concluding paragraphs of a chapter. The complicated multinational context of Croatian history is left confusing. The author's writing style is dull and studded with trite and outmoded usages (such as "viz."). The author has his own highly personal way of spelling non-Croatian names (for example, Kramarz, Husit Wars, Olumec, Sobiesky, Safaryk), and this is compounded by the almost complete absence of necessary diacritical marks throughout the book. In his introduction Gazi lists a number of published works on Croatian history that he has consulted. No additional bibliography or notes are included. Reference matter consists of a two-page chronological outline and six pages of crude maps. The only positive thing to be said of this work is that its author is not a Croatian chauvinist. Although his preferences for Catholics, Croats, and the peasant leader Radić are quite evident, they, like the traditional anti-Serb and anti-Magyar biases, are kept in reasonable check. A feeble vanity publication such as this contributes absolutely nothing to the pressing needs of American scholars and teachers involved with East Central European history.

JOSEPH FREDERICK ZACEK
State University of New York,
Albany

F. L. CARSTEN. *Revolution in Central Europe, 1918-1919*. Berkeley and Los Angeles: University of California Press. 1972. Pp. 360. \$11.95.

In many ways this work illustrates the historian's craft at its best. A talented and established scholar has addressed himself to a familiar period and subject, found new sources and explored them with the thoroughness of the most earnest Ph.D. candidate, and reinterpreted the events of the time. His stage is Germany, the German provinces of Austria, and to a lesser extent Bohemia and Hungary; the time period is approximately the year that followed the

defeat and dissolution of the armies and governments of the Central Powers. In a broad sense the drama is one of many social forces in contention with each other in a chaotic situation, but the author throws his spotlight on the activities of the soldiers' and workers' councils, which sprang up in so many cities and towns. As primary sources he has consulted the municipal records of Berlin, Vienna, and scores of provincial centers, supplementing them with memoirs and other material, with the result that he can show us, province by province, just what kind of revolution, counter-revolution, or nonrevolution was taking place.

Two striking conclusions about the councils come through. One is the degree to which they played a moderate and constructive role. Far from being revolutionary soviets they did a great deal to hold the fabric of society together, helping to maintain security, to distribute available food supplies equitably, to look after social welfare, and to enable local governments to function. The second notable point is that the councils were limited in their potential by the moods, traditions, and continuing political patterns of their respective localities. In Austria, bearing the imprint of the left wing of Social Democracy, they had no firm roots outside Vienna and Lower Austria, and even there they lacked the support of the Social Democratic leadership that was essential to their exercise of real political power. In Berlin and elsewhere in Germany they had a major influence on the composition of new governments, but unfortunately, in the author's view, they had little staying power because of their own internal divisions, the distrust of the middle class and the trade unions, and the attacks of militants on the extreme left and right. Carsten gives us a new interpretation of Kurt Eisner and his attempt to carry out a revolution in Bavaria based on strengthened workers', peasants', and soldiers' councils, and of the Councils Republic, which was proclaimed after his murder and soon fell into the hands of the Communists.

In his story and his conclusions the author interprets the events of 1919 as a series of lost opportunities for basic social change, with the result that democracy did not find a secure home in Central Europe. Thus the revolution "failed." Here the thesis runs into problems of definition, and, as he says, many may disagree.

But there is no denying that his is a fresh insight ably presented and backed by solid research.

JOHN C. CAMPBELL
Council on Foreign Relations

PETER F. SUGAR, editor. *Native Fascism in the Successor States, 1918-1945*. Santa Barbara, Calif.: ABC-Clio. 1971. Pp. 166. Cloth \$9.50, paper \$4.50.

This collective work owes its importance primarily to the fact that it is one of the very few treatments of East European history that attempts to be comparative. The authors were asked "to investigate what features of the fascism manifested in Eastern Europe differed from those observable elsewhere and what common characteristics these local manifestations had that could be tied, at least to some extent, to their common Habsburg inheritance" (p. 147). The fascist movement in each of six countries, furthermore, was subjected to analysis twice, once by a citizen of the country concerned and a second time by an American specialist. (Five of the Americans, however, were born in Eastern Europe.) The papers were first presented to a conference held in Seattle and organized by the University of Washington in 1966.

Yet the juxtaposition of authorities from two continents did not produce much in the way of contrast or controversy. (Nor is there much direct comparison within the area, not to speak of matching East European with fascist movements elsewhere.) Rather, the principal difference in interpretation that emerges cuts across the lines of citizenship and divides the authors into those who think fascism was not a very important force in their countries and those who think it was.

Thus, on the one hand, R. John Rath of Rice University asks whether it is fair to indict seven million Austrians for not resisting the might of the Third Reich when the Western great powers stood idly by, while Jan Hvránek of Charles University argues that neither Hlinka's nor Henlein's party was "truly fascistic" until after 1939 (p. 52). On the other hand, George Barany (Denver) speaks of a disease that "must have penetrated all strata of Hungarian society fairly deeply by the time of the Second World War" (p. 77), and Stephen Fischer-

Galati (Colorado) portrays the Iron Guard as the dominant force in Romanian politics in the 1930s, explaining that the Guardist vote in the election of 1937 was in fact significantly greater than that officially reported.

This division is in part confounded with another, which is the line between countries (or peoples) that could expect assistance from the Third Reich in the realization of state interests or of ethnic ambitions (the Hungarians or the Croats) and those who could not, for example, the Poles and the Czechs. Indeed, the first fascist-like parties arose among the German populations of Bohemia in the 1890s as a popular reaction to the advance of Czech nationalism. By the 1920s fascism appeared to be a single movement with Bohemian, Austrian, and German branches. Those of our authors who regard fascism as of little importance are also inclined to view it as Nazi-imposed or Nazi-supported, while those who think to the contrary see it as a nativist phenomenon with a life of its own.

But in part, at least, the division reflects a definitional problem, reference to which presents itself in almost every chapter. The editor asserts in his conclusion that the authors have achieved a tacit definition of fascism, one related to the totalitarian conception of Hannah Arendt, whereas the author of the introduction, Lyman H. Legters, maintains that "the fullest value of these studies can only be extracted by relating them to the as yet only incipient effort to refine the concept of fascism" (p. 9). I agree with Legters, and I would go so far as to say that the unexpected outcropping of the definitional problem is one of the more important results of this application of the comparative approach.

Native Fascism is also useful because it provides convenient summaries, often based on sources in generally inaccessible languages, and because it illuminates some problems common to the Habsburg successor states.

R. V. BURKS
Wayne State University

VICTOR S. MAMATEY and RADOMÍR LUŽA, editors. *A History of the Czechoslovak Republic, 1918-1948*. Princeton: Princeton University Press. 1973. Pp. xi, 534. \$22.50.

Few small countries in the twentieth century

have played a more important part in world affairs than Czechoslovakia, at one time the world's eighth industrial power and the only parliamentary democracy in Central and Eastern Europe. Continuing efforts by Czechs and Slovaks to build a democratic society have commanded worldwide attention, especially in 1918 and again in 1938, 1948, and 1968 as domestic conflict in Czechoslovakia led to an international crisis. To help inform a renewed Western interest in Czechoslovakia, a team of fourteen American and European scholars has taken advantage of new evidence and a greater perspective in time to produce a balanced and objective history of the Czechoslovak Republic in a European context. This work, ably edited by Victor S. Mamatey and Radomír Luža, and the equally recent study by Věra Olivová, *The Doomed Democracy: Czechoslovakia in a Disrupted Europe 1918-1938* (1972), are the most comprehensive and informed surveys of modern Czechoslovakia to date in English. Of these two works, the former is the more detailed and better documented and alone covers the critical and still controversial decade from Munich to February 1948.

Each topical chapter in *A History of the Czechoslovak Republic* is written by a specialist, and the work as a whole is characterized by a remarkably uniform excellence in style and scholarship. Every author has utilized available archival sources and contemporary Czechoslovak as well as Western scholarship. Two chapters by Victor S. Mamatey comprehensively survey the founding of the Republic and "the development of Czechoslovak democracy from 1920 to 1938." Václav Beneš presents the most thorough and balanced account in English of the postwar consolidation of the Republic during two years of domestic and international crisis. Piotr Wandycz cogently and critically evaluates all facets of Czechoslovak foreign policy from 1918 to 1938 and very fairly assesses its principal architect, Eduard Beneš. Keith Eubank reviews the Munich crisis from beginning to end with emphasis upon its international as opposed to its domestic aspects. J. W. Bruegel's sympathetic and objective discussion of "the Germans in pre-war Czechoslovakia" and Zora P. Pryor's thorough survey of "Czechoslovak economic development in the inter-war period" complete the series of seven articles on

the first twenty years of Czechoslovak independence.

Six chapters cover the period of Nazi occupation from 1939 to 1945. Jörg K. Hoensch presents the most analytical and objective survey of the quasi-independent Slovak Republic yet to appear in English. Theodor Prochazka concentrates upon the short-lived and heretofore largely neglected Second Czechoslovak Republic from Munich until its abolition by the Nazis. Balanced essays by Gotthold Rhode, Edward Taborsky, Radomír Luža, and Anna Josko examine respectively the Protectorate of Bohemia and Moravia, the Czechoslovak government in exile, and the Czech and Slovak resistance movements.

Political and economic problems in the post-war era and the expulsion of the Germans are surveyed in three chapters by Radomír Luža, Jan Michal, and Ludvík Němec. A concise epilogue by Victor S. Mamatey reviews the principal themes of the book and helps place each chapter in perspective. Readers wishing to undertake further study of modern Czechoslovakia will appreciate the forty-two page annotated bibliography by Radomír Luža; his judgments are fair and to the point, excepting several instances in which he has too negatively evaluated recent publications by Czech and Slovak historians.

A History of the Czechoslovak Republic is enhanced by a detailed index and is characterized throughout by clear prose, ample documentation, and very few factual or typographical errors. In general, its authors discuss domestic politics, foreign affairs, and economic development more thoroughly than other topics and no more than sketchily examine arts and letters, education, religion, and the institutional and class structure of Czechoslovak society. A short review of their fine work cannot adequately comment upon each chapter. Suffice it to say in conclusion that every scholar interested in modern Europe will find their book an invaluable reference work and a stimulating and informative narrative.

BRUCE M. GARVER
Yale University

J. W. BRUEGEL. *Czechoslovakia before Munich: The German Minority Problem and British Ap-*

peasement Policy. New York: Cambridge University Press. 1973. Pp. xiii, 334. \$14.95.

The bulk of Mr. Bruegel's book deals with the Munich crisis of 1938 and its immediate antecedents. The book also contains chapters on the formative years of Czechoslovakia and treats in relatively great detail Czechoslovak-Reich relations from 1918 to the rise of Hitler as well as the participation of German parties in Prague coalition governments. Bruegel notes that the Weimar Republic upheld no territorial claims against Czechoslovakia and that its diplomats even encouraged Sudeten-German politicians to cooperate with the Czechs. In the fact that from 1925 to 1938 it was indeed possible for Germans to be partners in Czechoslovak governments Bruegel sees proof that Germans were gradually abandoning their original negative position toward the state and that the German problem was being solved. In strong terms the author blames Britain's statesmen and diplomats for allowing Hitler to destroy Czechoslovakia and he attributes their actions to their stupidity, dishonesty, and callousness.

It is indeed true that for a considerable time a clear majority of the German electorate supported activist parties. By the spring of 1938, however, an equally clear majority voted for the Sudeten-German party, which was not loyal to the republic. Activists left the government, and their parties, with the exception of the Social Democrats (who also lost votes), were decimated. It is also true, of course, and Mr. Bruegel notes this, that even in 1938 about a third of the German votes were cast for German opponents of the Nazis. Does it mean, however, that they were also cast, as he alleges, in favor of the Prague regime? The answer has to be in the negative. Those Germans who recognized Czechoslovakia as a political fact did not extend their recognition to the conception of Czechoslovakia as a national state of the Czechs (and one in which Germans were a minority), a position Mr. Bruegel rightly attributes to the country's "powerful and generally chauvinistic bureaucracy." It is impossible to attribute the rise of the pro-Hitler element in Czechoslovakia solely to the intrigues or attraction of Hitler's Reich and to ignore the fact that German activism in Czechoslovakia failed to win those concessions that all Germans considered necessary.

It is even more wrong to ignore the fact that British policy makers considered the system under which the Czechoslovaks were a "nation of the state" and Germans a national minority untenable in the long run, Hitler or no Hitler. The earlier German version of this book (*Tschechen und Deutsche, 1918-1939* [1967]) devoted a great deal more attention to domestic affairs and thus made it easier to understand not only the complexity of Czechoslovakia's internal situation but also its influence on the policies of foreign powers, such as Britain. Hitler, whose plans regarding Czechoslovakia were not limited to territorial correction, but included eventual elimination of the Czech people from Central Europe, was able to take advantage of Sudeten-German grievances, whose theoretical validity even Prague recognized. The Sudeten Germans were willing in 1938 to identify their cause with the goals and policies of Hitler, thus making it impossible to preserve a reformed Czechoslovakia. In 1945 they in turn were treated as a part of the Nazi legacy, and their national existence in Czechoslovakia was terminated together with that of the Third Reich.

ROMAN SZPORLUK
University of Michigan,
Ann Arbor

I. I. POP. *Chekhoslovatsko-ungerskie otnosheniia (1935-1939)* [Czechoslovak-Hungarian Relations (1935-1939)]. (Akademiia Nauk SSSR, Institut Slavianovedeniia i Balkanistiki.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 245.

This is an interesting and valuable contribution to the history of Czechoslovak-Hungarian relations during 1935-39. The book is based mainly on published documents and secondary sources; however, some Czech archival materials are used, but outside of the TASS collection and some manuscripts no Soviet archival documents are utilized. There is no bibliography, and some important Hungarian, Czech, and Polish sources were not consulted. In addition to the skillful use of documents found in the *Diplomáciai iratok Magyarország külpolitikájához 1936-1945*, I. I. Pop should have also consulted *Magyarország és a második világháború. Titkos diplomáciai okmányok a háború előzményeihez és történetéhez* (1959), which contains some of Milos Kozma's papers relating

to the Carpathian Ukraine—or at least he might have utilized its Russian translations: *Vengriia i vtoraiia mirovaia voina; sekretnye diplomaticheskie dokumenty iz istorii kanuna i perioda voiny* (1962). Apparently among the Czech archival material he has missed “*Dokumenty z jednání o územních změnách Československé republiky, 19.IX.—30.IX. 1938.*” Its third volume of fifty-seven pages covers the Czech negotiations with Hungary. Polish publications are better utilized, but such important works as Stanisławska’s *Polska a Monachium* (1967) and Lipski’s *Diplomat in Berlin* (1968) should not have been overlooked. Furthermore a greater use of Western documents also would have been beneficial.

The most interesting and valuable parts of the book are those discussing the Hodža Plan, whereby it is shown that since 1936 Prague periodically tried to stabilize the Danubian region through closer economic and political relations with Hungary and Austria. Due largely, however, to the inflexible position of Budapest and the Hungarian insistence on territorial changes at the expense of Czechoslovakia all Czech attempts to improve relations with Hungary were without success. Failing to find strong supporters abroad for revisionist policies Budapest found itself isolated, and in the end Hungary was left totally dependent upon the favors of the Third Reich.

Specialists will find little that is really new here, but the book is a valuable contribution to historiography—and indeed it represents a new gender in Soviet historical writing! Naturally its presentation falls within the general Marxist-Leninist framework, but it is refreshingly void of the usual abundance of ideological jargon. The diplomatic entanglements are generally presented with a measure of objectivity, and the clarity of presentation and style are most captivating. Thus in a final analysis both specialists and students will find the book rewarding.

C. M. NOWAK

Bridgewater State College

WALTER R. ROBERTS. *Tito, Mihailović and the Allies, 1941–1945*. New Brunswick: Rutgers University Press. 1973. Pp. xv, 406. \$15.00.

The author, an associate director of the United

States Information Agency, has made an outstanding contribution to historical scholarship. His carefully researched book is an objective work on a complex and controversial subject and represents a turning point in the historiography of Balkan resistance movements during World War II. It depicts a Yugoslavia overwhelmed by Nazi military might, partitioned, occupied, and soon the scene of a bitter civil war between two resistance movements. The author does a superb job of summing up the major developments and how they were influenced by the decisions of the major allies.

Roberts shows how the Big Three acted in support of what they conceived to be their short-run interests (winning the war) and against their long-term goals (ideologically preferred political systems). For Churchill (and Roosevelt through Churchill’s urging), Tito’s Communist-led partisans were killing the most Germans and therefore should be supported. Stalin, on the other hand, believed that Yugoslavia could help most by avoiding civil war, urging Tito to assist Mihailović and his *chetniks*, who were the first to raise the flag of resistance. Only when it was too late did Churchill express concern about the political implications of his earlier decisions.

Perhaps the book’s greatest service is the light it throws on the leaders of the two resistance movements. Both wanted the Allies to win and assumed that they would. Tito’s main concern was that he be in power at the end. Mihailović, on the other hand, wanted to prevent that from happening. He would have preferred to follow Western Allied instructions to European resistance movements to lie low until the signal of Allied landings. But Tito’s actions forced him to fight against an imposed political solution, hoping that when the war ended the people would have an opportunity to determine their own destiny.

Roberts explodes the myth that Mihailović was a traitor and Tito a patriot. Each regarded the other as his principal enemy, and three of Tito’s top associates (including Milovan Djilas) even put their position in writing during a negotiating session with German representatives. The Germans were happy to see Tito and Mihailović fight each other, but they viewed both as enemies of the Third Reich and offered the same reward for their capture.

Specialists may wish that the author had given more space to certain subjects, and they may find minor faults, but they cannot deny that this is a first-rate book on a subject that has produced so much bad history over the past twenty-five years.

ALEX N. DRAGNICH
Vanderbilt University

IV. V. BROMLEI *et al.*, editors. *Slaviane i Rossiia: K 70-letiiu so dnia rozhdeniia S. A. Nikitina* [The Slavs and Russia: For the 70th Birthday of S. A. Nikitin]. (Akademiia Nauk SSSR, Institut Slavianovedeniia i Balkanistiki.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 304.

Slaviane i Rossiia is a *Festschrift* presented to the distinguished scholar of Russo-Balkan relations, S. A. Nikitin, on his seventieth birthday. If the *Festschrift* is generally a reviewer's nightmare the book under review is particularly so. It contains thirty-four items, some rather fragmentary, some quite substantial, on topics ranging from feudal rents in Dalmatian Croatia in the thirteenth and fourteenth centuries to the crisis over Trieste at the end of the Second World War. Only Professor Nikitin himself, perhaps, has the range to adequately criticize all the essays in *Slaviane i Rossiia*; I will confine myself to commenting on a few that seemed in one way or another noteworthy.

One of more interesting essays dealing with the earlier centuries of Russian and Balkan history is L. V. Cherepnin's discussion of grants of immunity and privilege among Russian and south Slavic princes in the fourteenth and fifteenth centuries. Cherepnin's most interesting point, which he merely poses and does not develop, pertains to the relationship between these grants and subsequent legal norms. The degree to which privileges and immunities subsequently acquire a more general legal application—or fail to do so—provides an excellent device for the analysis of the power relations between the prince and local aristocracies over time. It is a pity that Cherepnin did not discuss either the potential uses or the practical difficulties of this kind of analysis in more detail. In Russia, for instance, the number of codified legal sources for the period in question is so small that it would seem to make the kind of analysis that Cherepnin suggests very difficult.

The heart of *Slaviane i Rossiia*, as of Niki-

tin's published work, is the relations between Russia and the emerging Balkan nations in the nineteenth century; eighteen of the thirty-four contributions deal with some aspect of this massive topic. One of the best is a solid, judicious and well-grounded piece by I. S. Dostian, dealing with the attempts of Serbian nationalists to obtain Russian assistance in the founding of a Serbian state in the first years of the nineteenth century. K. L. Strukova's "On the Character of the Development of the European Provinces of Turkey in the First Three Quarters of the Nineteenth Century" contains valuable evidence of the backwardness of Macedonia. E. M. Shatokhina's essay on the socio-economic condition of the Bulgarian peasantry in the 1860s is based on information accumulated by Russian diplomats in Turkey and now located in the Archive of the Foreign Policy of Russia (AVPR).

Perhaps the most substantial essay in *Slaviane i Rossiia*, although not surprising in its conclusions, is N. M. Druzhinin's "The Agrarian Reform of 1866 and Its Consequences." Using the statistical survey of state properties (1858) and a government compendium of 1888, surveying the activity of the Ministry of State Properties over the preceding fifty years, Druzhinin shows that the bulk of the former state peasants were no better able to deal with their chronic shortage of land and heavy financial exactions than were the former landlord's peasants, despite the modest advantage over the latter that they had enjoyed at the time of the reform of 1866.

Several of the essays in this volume deal with the mutual relations between Russian radicals and Balkan revolutionaries. The majority are informative, but several suffer from excessive abstraction and schematism, as does V. D. Konobeev's treatment of the agrarian program of the Bulgarian radical, Vasil Levski. Konobeev is fond of such deadening sentences as: "Having correctly defined the final, objective goals of the peasant struggle, Levski selected the only true path to their resolution—the popular (*narodnyi*) path, that is, the bourgeois-democratic revolution, which must resolve the task of the social liberation of the peasantry."

If Konobeev's Marxism is abstract and conventional, A. S. Beilis's treatment of American missionaries in Bulgaria ("From the History of Early American Penetration in Bulgaria") is

angry and confused to the point of utter incoherence. Beilis accuses American missionaries in general of "ideological aggression," but does not define the term further and goes on to assert that American missionary activities in Bulgaria (1850-80) merely cloaked "intelligence and espionage activities." Needless to say this broad claim is not supported by any real evidence. What really appears to enrage Konobeev is that American missionaries (in his view) were systematically trying to undermine Russian influence in Bulgaria. Although he certainly exaggerates the directedness and consistency of American efforts he is probably right. But any consistent Marxist should applaud their efforts; surely in the middle of the nineteenth century the ideas emanating from Robert College and American missionaries were more "progressive" than those espoused by the Russian government.

Slaviane i Rossiia, then, is a grab bag. There are a number of excellent articles. There is considerable information on a variety of historical points of Russian and Balkan history. But *Slaviane i Rossiia*, like most *Festschrifts*, should be dipped into judiciously, rather than read cover to cover.

ABBOTT GLEASON
Brown University

A. A. ZIMIN. *Rossiia na poroge novogo vremeni (Ocherki politicheskoi istorii Rossii pervoi treti XVI v.)* [Russia on the Threshold of the New Age: Essays on the Political History of Russia in the First Third of the 16th Century]. Moscow: Izdatel'stvo "Mysl'" 1972. Pp. 451.

This latest work by one of the leading historians in the USSR is a survey of the reign of Vasilii III (1505-33). In recent years Soviet historians such as Zimin, Lur'e, Kazakova, and Klibanov have written on specific aspects of Vasilii's reign, their major efforts being studies of the dynastic crisis prior to Vasilii's accession, the question of the secularization of monastic estates, and heretical and rationalistic movements in the Church. These topics, in addition to discussions of internal politics and a heavy stress on foreign policy, constitute the bulk of Zimin's book.

Because Vasilii ruled during the early adolescence of the Muscovite Empire historians might expect to find in this work chapters on

class structure, economic development, and the growth of the administrative apparatus. Yet except for a number of pages on the economy these subjects are not adequately treated. The fault, however, is not entirely Zimin's, for the historical data of the period (chronicles and publicists' literature) tells us little of these matters. We have, therefore, a very unbalanced account of Vasilii's reign, one in which we do not even find a true biographical sketch of the ruler.

Zimin's work is based on a close study of the primary sources. But aside from some minor points that he disputes with other historians his overall interpretation is conventional: Vasilii's efforts at territorial expansion and concentration of political authority in the monarchy represented a continuation of his father's policies. One novel point that Zimin should have dwelt upon is his argument that Vasilii seriously contemplated leaving the throne to a Christianized Tatar prince and later to the man who married that prince's daughter. Considering Ivan IV's brief abdication to a Tatar prince in the 1570s one might speculate on Muscovy's image of the Chingisid dynasty in the sixteenth century. In terms of legitimacy it may well have been considered equal to the Riurikid. But Zimin rarely speculates on such a scale. In all his book is the best survey we can expect, considering the imbalance in the available data. Yet we shall remain frustrated in not knowing what actually went on in Moscow in the early sixteenth century.

THOMAS ESPER
Case Western Reserve University

EFFIE AMBLER. *Russian Journalism and Politics, 1861-1881: The Career of Aleksei S. Suvorin*. Detroit: Wayne State University Press. 1972. Pp. 239. \$12.50.

B. I. ESIN. *Russkaia dorevoliutsionnaia gazeta, 1702-1917 gg.: Kratkii ocherk* [Russian Prerevolutionary Newspapers, 1702-1917: A Brief Essay]. Moscow: Izdatel'stvo Moskovskogo Universiteta. 1971. Pp. 86.

B. P. BALUEV. *Politicheskaia reaktsiia 80-kh godov XIX veka i russkaia zhurnalistika* [The Political Reaction of the 1880s and Russian Journalism]. Moscow: Izdatel'stvo Moskovskogo Universiteta. 1971. Pp. 313.

While the peculiarities of prerevolutionary Rus-

sian journalism have inevitably received some attention outside the Soviet Union from specialists in literature and the history of ideas the role of the press in the political life of tsarist Russia has been given undeservedly little attention. Where it has been taken into account judgments about the significance of particular periodicals and journalists have all too often been taken at secondhand from tendentious treatments by Russians (for whom, both before and after 1917, it has been impossible to discuss this topic without a *parti pris*) or from a variety of unsatisfactory Western sources.

A welcome contribution to the inchoate Western effort in this field is Professor Ambler's substantial and lively study of A. S. Suvorin, a publicist and literary entrepreneur whose career spanned the period from the Great Reforms of the 1860s to the eve of the First World War and whose notably profitable abandonment of a moderate and eclectic liberalism in favor of the firmer ground of chauvinism and conservatism made him a by-word for journalistic venality. It is the course of Suvorin's political development that forms the framework of Professor Ambler's study: the period under consideration is the last twenty years of the reign of Alexander II, during which many of Suvorin's compatriots described a similar path, from the civic euphoria of the early 1860s to the indifferentism and reaction of the early 1880s. The conflict between this tendency and the persistent oppositional traditions of the intelligentsia is nicely handled by Professor Ambler, with a valuable emphasis on the role that the organs of the periodical press filled as a surrogate for normal political parties. Unfortunately Suvorin is not an entirely satisfactory point of focus for the examination of these questions, for reasons that Professor Ambler herself acknowledges: first, because we have uncommonly little material with which to investigate the private thoughts behind Suvorin's publicistic persona (his published *Diary* concentrates on a period beyond the limits of Professor Ambler's work and is in any case, as recent Soviet scholarship indicates, textually suspect), and more importantly, because his political views were too miscellaneous and contingent to offer much beyond a case study in the art of adaptability. Suvorin's drift to the right gathered speed notably after he

acquired his own newspaper in 1876, and one suspects that his was the example that moved the then chief of the censorship to remark that he was glad to grant permission for known dissidents to open new periodicals, since while they might "play the liberal on someone else's paper, with their own they have to look out for number one."

Nonetheless Professor Ambler has done an excellent job of describing the political and intellectual currents that created the milieu in which Suvorin worked and the dynamics of the relationship between the press, the government, and the tiny educated minority that regarded itself as the representative of "public opinion." Equally valuable is her repeated reference to moments in the material development of the Russian press, a banal detail often ignored in Western treatments, but indispensable for giving a sense of proportion to any discussion of the politics of Russian journalism. The only serious reservation to suggest itself concerns matters of translation. Professor Ambler tends to use excessively literal translations in contexts that are clear enough to the Russian specialist, but may confuse historians from other disciplines who ought not to be put off by such an instructive book. Thus, for instance, the repeated use of the word "accusatory" to describe authors and works from the vein of Russian social criticism that combines both exposé and remonstrance is too terse. Equally odd-sounding is the frequent use of "idea-less" to translate the word used by all hues of the Russian left to condemn any writing not informed with high-minded civic principles. These (and numerous other) dictionary translations are, however, tolerable faults in an extremely useful book.

B. I. Esin's short study of the development of the newspaper press in Russia is (as, indeed, are all his works) an example of the best in Soviet history of journalism and should be read by every scholar working on the political and intellectual history of Imperial Russia. In the hands of a historian of Esin's caliber the tools of Marxism are extremely effective for the exposition of problems that must be considered if a work in this field is to have more than an anecdotal or a narrowly technical value: the development of newspapers in any country is inextricably bound up with questions

of economic and social changes and their political consequences, and the narrow focus that Soviet historians of journalism bring to bear on these questions supplies a solid base from which scholars free of ideological imperatives may range with much greater variety and penetration, as has Professor Ambler. Another instructive point that arises from a consecutive reading of these two books is the reminder that the curious notions that many Russians have about the purpose of journalism have changed little since Suvorin's day. Despite Esin's sobriety, thoroughness, and reliability in matters of fact it is always evident that he cannot begin to sympathize with a system of journalism in which diversity, competition, and spontaneity are values of the first importance.

This may be said even more emphatically of B. P. Baluev, whose study of the press in the reign of Alexander III is a model of the grotesque vulgarization of history that can be produced by writers whose use of Marxism is arrested at the level of political agitation: Baluev's work is rubbish both as history and as polemic. Only the narrow specialist would enjoy a detailed exposition of his errors of fact and the nonchalant violence that he works upon his sources. Suffice it to say that the book can only be read as a sort of historical mirror-writing. Even as polemic it is ridiculous, written as though bands of Cadets were about to leap out of every Moscow sidestreet, laying about them with sheaves of statistics, quill pens, and other frightful weapons. One must hope that Baluev will quickly find his true *métier* as a writer of *feuilletons* for a party newspaper in one of the Soviet Union's duller provinces. Fortunately this work is an anachronism; most Soviet historians of journalism are doing excellent work, and the field of Russian studies will be notably enriched if more Western scholars follow Professor Ambler in using their techniques as the starting point for a fresh approach to the history of tsarist Russia.

DENNIS M. O'FLAHERTY
St. Antony's College,
Oxford

D. L. BESKROVNYI, *Russkaia armii i flot v XIX veka: Voенно-ekonomicheskii potentsial Rossii* [The Russian Army and Navy in the 19th Century: Russia's Military-Economic Potential].

(Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1973. Pp. 615.

In this substantial volume the author, one of the leading Soviet military historians, seeks to show the problems of Russian economic and military potential and how its defenses developed from a war machine based on serfdom and a backward industrial and transportation system to one with universal manhood service, a largely modern navy, an extensive railway network, and powerful industry. Earlier Russia had to make extensive changes in its war machine to cope with the French, such as introducing divisions, army corps, and a more rational command. But serfdom, which restricted military manpower and drew its officers solely from the nobility, prevented any real modernization of the military and naval services. Beskrovnyi approaches his subject in a series of essays, each of which deals with a separate aspect of the problem expressed in abundant and detailed statistical tables.

Because of the backwardness of the social system Russia was at a grave disadvantage in the Crimean War, which compelled sweeping reforms: in railways, modernization of weapons, military education, and the introduction in 1874 of universal military service. Industry, much of which was state-owned, had to be greatly expanded to provide the steel and breech-loading rifles and artillery, the smokeless powder, and the armored vessels and torpedo boats to keep pace with the rest of the world. Russian science, called on to provide technical assistance, proved able to meet the demands placed upon it. All this, however, had important consequences, for as the non-noble classes supplied more and more of the officers they took an active role in the revolutionary movement, which continued to grow. Moreover, the enlisted men, given a modicum of education, were vulnerable to propaganda, which in part was introduced into their midst by conscripted workers. Thus, in bringing the masses into the armed services, the autocracy was admitting a Trojan horse that was to bring about its ruin.

This volume is by no means easy reading, partly because of its organization. It is, however, so convincing in its mass of detailed archival evidence that it is a documentary

exposé that no one who deals with Russian defense matters in the nineteenth century can afford to overlook.

JOHN SHELTON CURTISS
Duke University

KHADZHI MURAT IBRAGIMBEILI. *Kavkaz v Krymskoi voine, 1853-1856 gg., i mezhdunarodnye otnosheniia* [The Caucasus in the Crimean War, 1853-1856, and International Relations]. (Akademiia Nauk SSSR, Institut Vostokovedeniia. Azerbaidzhanskii Gosudarstvennyi Universitet im. S. M. Kirova.) Moscow: Izdatel'stvo "Nauka," Glavniia Redaktsiia Vostochnoi Literatury. 1971. Pp. 402.

A growing number of historical monographs have been appearing from the universities of the non-Slavic republics of the USSR. This one, from the Azerbaijani State University, exemplifies the problems that non-Slavic scholars face in the Soviet Union and the contributions to scholarship that they can nevertheless provide.

The author, an Azeri, is well acquainted not only with the usual literature, Russian and Western, on the Crimean War, but also archival materials from the Caucasian republics and published Turkish sources and accounts. With his unusual linguistic abilities the author is able to direct our attention to an important yet neglected part of the Crimean conflict, the military front between Russia and Ottoman Turkey in the Caucasus. Beginning with a long and relatively complete historiographical analysis of Russian-Ottoman relations in the Caucasus, which I think is the most valuable section of the book, Ibragimbeili proceeds to discuss, year by year, the events on the Caucasian front. More importantly the author includes discussions of local Caucasian political and military movements in his analysis of broader questions of international relations. In so doing Mr. Ibragimbeili presents an interesting revision of traditional Soviet historiography on Shamil and his rebellion against tsarist authority. Contradicting the usual view of Shamil as a reactionary Muslim fanatic he characterizes Shamil's movement as one for "national liberation" and relates it to Ottoman-Persian-Russian competition for hegemony in the Caucasus.

It is clear to the reader that Mr. Ibragimbeili feels uncomfortable with this position. One

notices, for example, the author's need to quote extensively from Marx, Engels, and Lenin to a much greater degree than a Russian historian would do today. In addition some of his most interesting ideas are so buried in traditional and rhetorical verbiage that only a careful reading can bring them to light.

Nevertheless this monograph is a welcome sign of major changes occurring in the Soviet historical profession, particularly in those branches in the smaller republics. For historians of the Crimean War it is a very important addition.

ALAN W. FISHER
Michigan State University

B. G. LITVAK. *Russkaia derevnia v reforme 1861 goda: Chernozemnyi tsentr, 1861-1895 gg.* [Russian Villages during the 1861 Reform: The Black-Soil Center, 1861-1895]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1972. Pp. 422.

The process of serf liberation in Russia involved two documents. The first was a contract (*ustavnaia gramota*) identifying the land allotments the ex-serfs were to receive and establishing the amount of rent they had to pay. The second was the "redemption act" (*vykupnyi akt*) by which the villages actually received their allotments in ownership and undertook to pay the government for them. The contracts were drawn up and signed in the early 1860s. Some of the acts were completed at about the same time, but most took longer. When in the 1880s the government ordered the last of them to be completed about one-fifth of the serfs had still not taken over their allotments. Litvak tells us that these documents—the contracts and acts—are the best source for studying the evolution of peasant landholding in nineteenth-century Russia. He ought to know; he has been working with them for almost twenty years. This book presents an exhaustive analysis of them in six *gubernias* of the central black-earth region of European Russia: Voronezh, Kursk, Orel, Riazan, Tambov, and Tula.

Litvak is the first scholar to make such an analysis on a large scale. It leads him to conclusions that differ with many hitherto prevailing views. He says, for example, that the increase in the number of household serfs on *barshchina* (*corvée*) estates during the early nine-

teenth century did not signify the rise of gentry luxury. Most household serfs were skilled laborers and the increase in their number probably reflected a growing spirit of economic enterprise. Further Litvak points out that *barshchina* estate owners were not taking away land from their serfs in the decades prior to the liberation. They did not even take as much land from them in the course of the liberation as did the *obrok* (money rent) estate owners. As for the *obrok* estates, rates of payment rose during the early nineteenth century, but serf income rose faster. Generally speaking Litvak concludes that gentry exploitation of the peasants before and after the liberation was not a matter of crushing or oppressing them, but of maintaining them in a condition that would allow them to work effectively.

Litvak is more cautious with his statistics than any historian of serfdom I have read. He qualifies most of his generalizations with whole catalogs of local exceptions. He does not insist on what his statistics prove so much as what they do not indicate. Litvak notes, for example, that gentry indebtedness in the 1850s was much higher on prosperous estates involved in market relations than on smaller, more primitive economies. However parasitic the serfowners may have been their debt did not reflect their insolvency or the economic unworkability of serfdom.

There are questions Litvak ignores. He takes no great interest in the fate of the gentry during the liberation; only in the ex-serfs. Moreover he leaves out any consideration of peasant income in the decades after the liberation. The questions he does ask, however, he answers convincingly despite the usual admixture of leninical exegesis. His scholarship is in the best Russian tradition.

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N. A. TROITSKII. "*Narodnaia Volia*" *pered tsarskim sudom, 1880-1891 gg.* ["The People's Will" before the Tsarist Courts, 1880-1891]. [Saratov:] Izdatel'stov Saratovskogo Universiteta. 1971. Pp. 238.

The 1880s marked an extended pause in the tempo of the Russian revolutionary movement. The populist faith in a peasant uprising had been shattered, society lay quiescent under re-

doubled government repression, and Marxism was still in its "gestational period." One force alone actively opposed the government, the dwindling and increasingly disorganized party of the "People's Will" (*Narodnaia Volia*). Troitskii's statistics show that members of *Narodnaia Volia* were defendants in all but 32 of the 103 political trials held in Russia between the middle of 1880 and the beginning of 1891. The leaders of *Narodnaia Volia* sought to turn these trials to the advantage of the party and by programmatic statement, agitation, or defiant gesture, made their day in court their last revolutionary act. Drawing on material from a variety of published and archival sources Troitskii reveals the party's program and ethic through its struggle with an increasingly baleful and arbitrary judicature.

What is most interesting about this book is Troitskii's assessment of the role that *Narodnaia Volia* played in the history of the Russian revolutionary movement. He ascribes to the party a greater stature and significance than recent Soviet scholarship has allowed. For Troitskii *Narodnaia Volia* is not a last bankrupt manifestation of populism, but an integral stage of revolutionary development marking a transition from populism to the new modes of opposition that will develop in the 1890s. Dealing with the period 1879-82 Troitskii specifically challenges S. S. Volk's contention that the tactic of political terror was a "fatal mistake" and argues persuasively that within the specific historical context of those years terror was not only unavoidable but correct. Troitskii's treatment of *Narodnaia Volia* during its years of disintegration after 1882 is equally provocative. He emphasizes those aspects of the party's organization and tactics that tend to link it as a precursor to the later development of Russian Marxism.

DONALD SENESE
University of Victoria

IA. S. KHONIGSMAN. *Pronyknennia inozemnoho kapitalu v ekonomiku Zakhidnoi Ukrainy v epokhu imperializmu (do 1918 r.)* [Penetration of the Economy of Western Ukraine by Foreign Capital in the Era of Imperialism (till 1918)]. Lvov: Vydavnytstvo L'vivskoho Universytetu. 1971. Pp. 253.

Novel in this study of economic history of the northeastern corner of the Austro-Hungarian

Empire in the period between 1890 and 1918 is the author's statistical appraisal, partly based on local archives, of a relatively high level of development of industries, banking, and especially of the joint-stock enterprises and cartels, in comparison not so much with the rest of Austria-Hungary as with the rest of the Ukraine in the Russian Empire. In the existing historiography on the capitalist economy in the Ukraine, to which this monograph contributes, predominant so far has been the view that the Austrian Ukraine was an extremely backward, overpopulated, agrarian region, with only a scattering of significant industries, such as petroleum and timber. Jacob Samuel Khonigsman shows now, however, that by 1912 industry finally came to produce slightly more of the local GNP than did agriculture, so that at least structurally the region was not much less developed than the rest of the Ukraine. There was indeed a very belated but genuine industrial revolution in eastern Galicia, Bukovina, and Trans-Carpathia, and decisively instrumental in it was the Rothschildian, German, Austrian, and some other multinational West European capital, particularly financial. Khonigsman estimates that in 1912 all foreign (i.e., in his definition, nonlocal) capital in west Ukrainian industries (excluding railroads, banks, and trade) amounted to 596.7 million crowns, or 73 per cent of total local working capital. This is a large figure if one compares it with about 550 million rubles of foreign capital spread throughout the whole of the Russian Ukraine (one crown was equal to 0.39 rubles). Following Lenin's definition Khonigsman views western Ukraine as an "internal colony" of Austria-Hungary (why not of the whole Western Europe?). One may still wish that his thesis concerning economic exploitation be better evidenced statistically, particularly through estimates of the west Ukrainian balances of trade and payments for longer periods of time. Had Khonigsman researched customs and banking archives in Vienna this task could probably be a feasible one. Also in a monograph dealing with Austria a German language summary would have been more useful for the international reader than only the Russian one.

VSEVOLOD HOLUBNYCHY

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M. P. SANAKOEV. *Deiatel'nost' krest'ianskogo pozemel'nogo banka v Gruzii, 1906-1917 gg.* [The Activity of the Peasant Land Bank in Georgia, 1906-1917]. (Akademiiia Nauk Gruzinskoi SSR, Iugo-Osetinskii Nauchno-Issledovatel'skii Institut.) Tbilisi: Izdatel'stvo "Metsniereba." 1971. Pp. 109.

This little volume examines the reasons for the establishment of the Peasant Land Bank in Georgia and its activities. The bank was set up in 1883, but extended to Georgia only in December 1905. The bank's main purpose was to facilitate the purchase of land by peasants. In practice, Sanokoev argues, the bank served the interests of the nobles by providing a mechanism for selling their land advantageously and provided the state a medium for implementing policies. As the period of its Georgian activity coincided largely with the years of the Stolypin land reforms it worked to encourage personal landowning at the expense of communal holdings.

The heart of the book is a study of the activities of the bank, replete with statistical tables. These examine the amount of land bought and sold, amounts paid, loan terms, and debt arrears. The second half of the book is made up of short essays on special topics about the role of the bank in the implementation of Imperial policies, especially the Stolypin reforms; the settling of Russian peasants in the area; the liquidation of German landholdings in World War I; and a brief but tendentious account of the period of Georgian independence. The data, though valuable, are difficult to use because the author constantly switches from Georgian to Transcaucasian or other regional bases for his figures. Moreover the book fails to elaborate on the social impact of the bank's activities. A glimpse is provided here and there, as in a chart showing shifts in landowning by nationality, but these features quickly slip past. Not only is the human factor absent, but the author does not even consider what role the Revolution of 1905 might have had on the decision to establish the bank. The broader perspective is sadly lacking.

While the proportions and aims of the book are modest it is useful to have such studies as a balance to the Great Russian orientation of most work concerning agriculture and the peasants, even if the author fails to make com-

parisons. It will be of interest to scholars studying a variety of topics: the peasantry, the Caucasus area, the nationality question in Imperial Russia, the Stolypin reforms, to name only some of the most obvious.

REX A. WADE

University of Hawaii

N. VALENTINOV (VOL'SKII). *Novaia Ekonomicheskaiia Politika i krizis partii posle smerti Lenina: Gody raboty v usikh vo vremia NEP vospominaniia* [The New Economic Policy and the Party Crisis after the Death of Lenin: Reminiscences of My Work at the VSNKh during the NEP]. Edited by J. BUNYAN and V. BUTENKO. With an introduction by BERTRAM WOLFE. (Hoover Institution Foreign Language Publications.) Stanford: Hoover Institution Press, Stanford University. 1971. Pp. xix, 256. \$9.50.

V. P. DMITRENKO. *Torgovaia politika Sovetskogo gosudarstva posle perekhoda k nepu, 1921-1924 gg.* [The Trade Policy of the Soviet State after the Transition to NEP, 1921-1924]. (Akademiia Nauk SSSR, Institut Istorii SSSR.) Moscow: Izdatel'stvo "Nauka." 1971. Pp. 269.

Dmitrenko's monograph on the commercial policy of the Soviet government from 1921 to 1924 offers little new on the subject, though he fills in an occasional detail. He is most interesting when describing the breakdown of Bolshevik resistance to the replacement of the state allocation of goods by a system of regular trade, which struck many Bolsheviks as an abandonment of the cause of the Revolution. But the author's obsession with the role of Lenin is debilitating and detracts from the value of the book, as does his distortion of the views of such people as Bukharin and at least one reference to Soviet archives for material readily available in print.

Valentinov's book is a different proposition since for the most part it is archival material in its own right. In the decades before his death in 1968 Valentinov devoted himself to reminiscences. In 1953 he published his memories of *Encounters with Lenin* (translated in 1968), relating to the time of the party schism. The book gave probably the only recognizably human portrait of Lenin that we possess. The present volume is devoted to portraits of other postrevolutionary luminaries, especially Dzerzhinskii, as head of the Soviet industrial or-

ganization, and Piatakov, his antagonistic lieutenant.

Valentinov was one of a number of non-Bolshevik intellectuals who, between 1923 and 1925, agreed to serve the Soviet state because they thought the regime had divested itself of the radical utopianism of the civil war period. They were also moved by the thought that no ready alternative was available if they wished to serve the interests of their country. Valentinov and a group of friends formed the "League of Observers," which shared a general perspective and pooled information and gossip that came its way from government and party officials with whom its members associated. In this volume Valentinov recreated from memory the activities and findings of the League. His memory was indeed colossal and, when it can be checked, accurate. Problems arise when corroborative information is lacking or when he gets so carried away by a concrete detail that the reader is hard put to know how important the detail may be. (Admittedly, if, as Valentinov reports, a special secret commission was formed in the Central Committee of the Communist party to investigate whether Lenin died of syphilis, perhaps the history of the Soviet higher circles really should be written in terms of small-town gossip-mongering.)

As a non-Bolshevik specialist, in his case an economic journalist, Valentinov fell in love with Dzerzhinskii when the latter moved from the Cheka to the VSNKh. By 1923 Dzerzhinskii had become a fanatical moderate (expressing his opposition to the industrialization schemes of the left wing with temperamental fanaticism) and treasured non-Bolshevik specialists who could perform. By the same token Valentinov's villains were the left opposition—Preobrazhenskii, Trotsky, Piatakov—in whom he saw too much civil war radicalism. His relations with Piatakov sound very odd since Piatakov seems to have made him something of a confidant, a role that Valentinov accepted.

In his book Valentinov argues openly, as he had before privately, that the New Economic Policy (NEP) provoked a tremendous opposition within the Communist party, that Lenin had to exercise all his authority to prevail, and that even after 1921 important segments of the party remained hostile to the principle of the NEP. Implicitly, Dmitrenko makes the same

case. But the evidence remains puzzling. If anyone had protested to NEP in 1921 the protest was denied in 1926-27 in public party gatherings. And had there been any shred of evidence showing that friends of Trotsky and Zinoviev objected in 1921 to NEP it would certainly have made big news in 1926. That it never did suggests the need to keep a degree of skepticism in accepting the thesis of widespread hostility to NEP at its inception among Communist leaders.

Altogether Valentinov's work is a lively, interesting description of personalities, which supplies a missing element when considering the 1920s. His description of the inner workings of the Special Conference on the Reproduction of Basic Capital (OSVOK) is particularly enlightening. The editors and the Hoover Institution are to be thanked.

DANIEL MULHOLLAND
Tufts University

NEAR EAST

ROBIN E. WATERFIELD. *Christians in Persia: Assyrians, Armenians, Roman Catholics, and Protestants*. New York: Barnes and Noble. 1973. Pp. 192. \$11.75.

Students of Iranian civilization have almost invariably disregarded the fact that Iran has always been a plural society composed of several distinct religious and cultural personalities. One reason for this oversight has been the tendency for Iranian cultural identity to be co-opted by the dominant religious creed of any given period. Thus, under the Sassanids, to be an Iranian meant to be a Zoroastrian; under the Safawids and the Qajars, to be an Iranian meant to be Shi'a Muslim.

For the historian the problem of defining the ongoing relationship between majority and minority communities is peculiarly baffling, whether the relationship is defined in terms of day-to-day social and economic intercourse or in terms of the more subtle symbiotic processes of acculturation. Even during the nineteenth century, a period when data relating to the minorities are most abundant, it is difficult to determine what impact, if any, the existence of Armenian and Nestorian Christian, Jewish, and Zoroastrian communities exercised upon

the Muslim majority. Of these minorities, the Armenians must have made a substantial, if unobtrusive, contribution to the molding of Iranian civilization, especially in the period when the Armenian kingdom constituted a formidable buffer protecting the northwestern marches of Arsacid and Sassanid Iran. Was there an Armenian element within the Iranian architectural tradition? Did the survival of Christian minorities during the early centuries of Arab domination contribute to the luxuriant growth of sectarian movements on the Iranian plateau? What was the effect upon the ethnic composition of Islamic Iran of a slave trade in Armenian and Georgian Christians, especially women, who were still being brought annually into Iran down to the first half of the nineteenth century? There can hardly have been a prominent family in Qajar Iran that did not somewhere have Christian Armenian or Georgian forebears.

To pose these questions is to indicate the limitations of Robin Waterfield's *Christians in Persia*, a most readable account that concentrates on the rather modest activities of nineteenth- and twentieth-century Catholic and Protestant missionaries in a country that shelters two of the most ancient Christian communities in the world, communities whose cultural traditions are hardly mentioned here. This is essentially an account of missionary activity, mainly from a missionary point of view. It is sometimes rather naive, occasionally inaccurate with regard to the broader currents of Iranian history, but it also contains much interesting information that, unfortunately, is unannotated. It does not take cognizance of the way in which Iranian nationalism, like other Asian nationalisms, regarded the missionaries as part of the larger intrusion of European imperialism, but it rightly records the devotion of a handful of men and women, "narrow, earnest men, but chiefly earnest," who made a substantial contribution to the educational and medical history of modern Iran, a contribution all too often overlooked.

This is not a definitive history of Christianity or even of European missionary activity in Iran, but most specialists in recent Iranian history will probably want to refer to it. The author may have overextended himself in taking on a subject of such dimensions. Pos-

sibly he should now narrow his range and attempt a detailed account of the activities of the Church Missionary Society in Iran from 1869. The subject deserves a chronicler, and Mr. Waterfield seems well qualified for the role.

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Yale University

JOSEPH J. MALONE. *The Arab Lands of Western Asia*. (The Modern Nations in Historical Perspective Series.) Englewood Cliffs, N.J.: Prentice-Hall, 1973. Pp. x, 269. Cloth \$8.95, paper \$4.95.

The Modern Nations in Historical Perspective series is designed to summarize "the chief historical trends and influences that have contributed to each nation's present-day character, problems, and behavior." Such a task is indeed difficult in a region in which more than one state is of questionable viability, and the concept of the nation-state itself does not flow in every case from the historical tradition.

Joseph J. Malone has made the best of the charge in *The Arab Lands of Western Asia*. Here are lucid accounts of the independence years of Lebanon, Syria, Iraq, Jordan, and the various polities of the Arabian Peninsula. There runs throughout a theme of diversity derived from unique historical roots, an emphasis that brings forth details of local history seldom included in surveys of this sort.

Yet this book has serious problems that render its overall utility questionable. In most cases the treatment of the preindependence years of the polities discussed is so compressed that events are referred to, rather than described. Hence its readership becomes in effect restricted to those already acquainted with Middle East history.

As the title suggests, not only is Israel omitted but also Egypt and North Africa. Denied more than half the Arabic-speaking world by previous volumes in the series, Professor Malone has found it necessary to depreciate the force of the Arab nationalist movement, however chimeric its goal may be. Egypt, the principal protagonist of that ideal in the Nasser years, is thus portrayed as a factor external to the region, despite its constant intrusion into the pages of the volume. Such an interpretation only serves to em-

phasize the artificiality of the concept of "the Arab lands of western Asia."

The focus of the series on the contemporary nation "in historical perspective" creates anomalies with which Professor Malone must wrestle. Lebanon, Syria, and Iraq as national entities with historic roots have a certain validity, but to put Jordan on the same plane and link it with Nabataea and the trans-Jordanian Biblical kingdoms implies a nationality, although the author does not specifically claim it. Turning to the Arabian Peninsula, Professor Malone faces a new difficulty with the concept he must manipulate. It is an area really quite separate from that which he has already discussed in separate chapters, one with a common history of insularity and Islamic traditionalism. It thus becomes a single chapter, nearly half the book. Yet the concept of the nation so dominates the structure of the volume that separate sections must be devoted, for example, to the two Yemens. Only when the Persian Gulf region is reached is the principle sacrificed to common sense, for the sheikdoms are hardly nations by any definition.

Finally, the state-by-state categorization has been extended to the index with ludicrous results: such stalwart pan-Arabicists as Michel Aflaq and Sati' al-Husri may be found only under the rubric "political leaders" for Syria and Iraq respectively.

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ISAIAH FRIEDMAN. *The Question of Palestine, 1914-1918: British-Jewish-Arab Relations*. New York: Schocken Books, 1973. Pp. xiii, 433. \$12.00.

Making use of previously unavailable British cabinet and Foreign Office papers at the Public Record Office, Isaiah Friedman has set himself an ambitious project: to unravel the tangled skein of pronouncements, exchanges, and deals involving the British government in the Middle East in World War I. In successive chapters he deals with various aspects of the personalities, events, expectations, and circumstances behind the Balfour Declaration, the Husain-

McMahon correspondence, and the Sykes-Picot Agreement.

The book has great merit. It is a detailed and revealing record with copious notes. Indeed, the notes are perhaps the most valuable contribution, for the reader now has a ready index to the newly available materials. Moreover, in attempting to deal with the whole range of issues, Mr. Friedman usefully juxtaposes the choices, dilemmas, and options so that each illumines the others. In the past, most authors have not even attempted this.

The book, however, fails to meet its objective in three important, even crucial ways. First, it does not set the stages or even the main stage upon which the British statesmen had to operate, nor does it suggest the circumstances in which the decisions were made. The First World War was vast and hard-fought beyond the worst dreams of those in power when it began; yet one senses a sort of de-natured, even ethereal, approach to events and decisions, an approach that was far from reality. Second, an order is imposed upon events and decisions, and it significantly distorts the turmoil of the war and the *ad hoc* nature of the decision-making process of all governments and especially of the British government of that period. Third, so neatly does he make the Balfour Declaration, the Sykes-Picot Agreement, and the Husain-McMahon correspondence fit together that there is no disparity to worry about and consequently no "problem." Too bad the British statesmen did not have Mr. Friedman's book to satisfy their worries. Lord Curzon flatly contradicted Mr. Friedman's thesis on December 5, 1918; and Mr. Balfour on August 11, 1919, warned his cabinet colleagues that "so far as Palestine is concerned, the Powers have made no statement of fact which is not admittedly wrong, and no declaration of policy which, at least in letter, they have not always intended to violate." Earl Grey expressed similar if more guarded thoughts in the House of Lords years later. Of these worries and doubts Mr. Friedman takes no note. In consequence one comes inevitably, I believe, to the conclusion that what he has prepared is a legal brief rather than a historical analysis. Unfortunately, as Mr. Friedman's approach to his subject shows, he is wrong in saying that "today it [the

Palestine question] has no more than an academic value . . . [while] in the past the keenest brains in Middle East politics were pitted against each other to prove their respective cases, only to demonstrate the gulf that separates them." But this study is another step toward the writing of a history. Taken together with other recent steps, notably Doreen Ingram's *Palestine Papers, 1917-1922* (London, 1972), this work offers the hope that another fifty years may not pass before we can look forward to a balanced, synthetic analysis.

WILLIAM R. POLK

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AFRICA

C. C. STEWART, with E. K. STEWART. *Islam and Social Order in Mauritania: A Case Study from the Nineteenth Century*. (Oxford Studies in African Affairs.) New York: Oxford University Press. 1973. Pp. xviii, 204. \$14.50.

When my *Mauritania: A Survey of a New African Nation* was published (1967), I noted in the introduction that my objective—to write a history of that country—was thwarted by the lack of historical analyses of segmentary societies there, and I called on other scholars to fill that gap systematically. *Islam and Social Order in Mauritania*, an adaptation of C. C. Stewart's Ph.D. dissertation, is an excellent contribution to that proposed, indispensable, bricklaying scheme.

The particular concern of the book is the career of Sheik Sidiyya al-Kabir (1775-1868), a venerated Mauritanian Qadiri mystic and jurist who, by the mid-nineteenth century—a century of intense Islamic activity in West Africa—was a major figure in religious, political, and economic affairs on the right bank of the Senegal River.

A presupposition carried over into West African history from Middle Eastern writing is that of the polarity of *figh* and *Tasawwuf* in Islam and that this culture is almost by definition an urban phenomenon. Prior to this century, however, this polarity was a negligible one in West Africa, and most highly respected jurists, like Sidiyya al-Kabir, also enjoyed very large followings as mystics of a Sufi brotherhood. This study painstakingly attempts to

explain this deviation from the norm. It also seeks to answer, in the context of nineteenth-century Mauritanian Gebla, such questions as, What were the roots of a possible connection between legal reforms and mysticism in the literature or social structure of influential Muslim communities in West Africa? What was the precise motive of the relationship between these themes of legal reforms and mysticism? How can one explain the presence of the Qadiriyya brotherhood in the greater part of the states and communities established by West African Islamic reformers? And what role did the Kunta scholars of Timbuktu play in the dissemination of that tariqa?

The authors were not only fortunate in their efforts to gain some historical perspective on the period because of a rich literary tradition in nineteenth-century Mauritania, but also because of the judicious analysis of these sources and the invaluable assistance they sought from such Mauritanian scholars as Moktar ould Hamidoun, Mohammad ould Mouloud ould Daddah, and others. I also heard a lot of positive comments about C. C. Stewart from a wide variety of Mauritanian intellectuals while visiting there last summer. But the authors evidence some lacunae in their understanding of Moorish society—at least this is suggested by the very skimpy section, “The Peopling of Mauritania,” which does not do justice to the very rich social patterns of that country. There is, for instance, a lack of distinction made between the nobility of Arab origin and that of Berber origin. Furthermore, the terms *zenagha* and *lahma* are not necessarily interchangeable.

Elsewhere the authors refute the contention that the war of Shurr Budda, a watershed in the history of Shingit, referred to in my book as “Mauritania’s Thirty Years War,” was a conflict in which the Berber autochthonous rose up in a final stand against the Hassani intruders, but they do not offer any other alternative reason.

Be that as it may, the Stewarts drew many conclusions about the practice of Islam in the Mauritanian Gebla and also raised many questions that may be taken up by other researchers; among the former is that by the late 1860s the most profound changes taking place there were not in response to an external (French)

power, but rather the result of the emergence of an individual, Sheik Sidiyya al-Kabir, and a people, the Awlad Ibiri, who bridged traditional hassani and zawiya divisions in Moorish segmentary society. On the whole, this excellent book is a welcomed rare addition to the history of West African Islam from its most important source—Mauritania.

ALFRED G. GERTEINY

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JOHN DE ST. JORRE. *The Brothers’ War: Biafra and Nigeria*. Boston: Houghton Mifflin Company. 1972. Pp. 437. \$10.00.

ALAN FEINSTEIN. *African Revolutionary: The Life and Times of Nigeria’s Aminu Kano*. [New York:] Quadrangle/The New York Times Book Company. 1973. Pp. xvi, 299. \$9.95.

R. A. ADELEYE. *Power and Diplomacy in Northern Nigeria 1804–1906: The Sokoto Caliphate and Its Enemies*. (Ibadan History Series.) [New York:] Humanities Press. 1971. Pp. xvi, 387. \$10.00.

The Brothers’ War is not the first and certainly will not be the last attempt to put the events of the Nigerian-Biafran conflict into an adequate perspective. Scanty coverage of the war by U.S. news media makes the effort welcome. Earlier publications have been mainly documents with annotations or involved persons’ interpretations. John de St. Jorre’s account is first-rate journalism; he covered the war, visiting both sides, after experience in war theaters in Southeast Asia, and now he has drawn together his own and other reports, official documents, background materials, and reflections and has produced a very readable account. He professes to be impartial, and no doubt he intends to be, but there seems to be a preference on his part for the principal of federalism rather than secession and the fact that the secessionists lost tends to give a certain coloring to his presentation that may make a close reader question somewhat the fullness of this impartiality. The personalities of the opposing leaders in de St. Jorre’s pages tend to forecast in the manner of a Greek drama the ultimate conclusion: Ojukwu is egotistic; Gowon has humility; the former tends toward autocratic decisions, the latter is consultative.

Beyond the history of the military events

and the ideological forces behind them *The Brothers' War* also reveals how the political structure of Nigeria has changed because of this experience. Gowan, finding himself in charge after the second coup (in which he had not participated), had a very uncertain basis of power. He demonstrated a shrewd political sense and an ability to win adherents. Being from a Middle Belt minority people he had empathy with the minorities in all regions who aspired to escape the domination of the major ethnic groups. The creation of twelve states in place of the old three regions strengthened his position during the war and has consequences for the future political complexion of the country.

Aminu Kano benefited from this move by Gowan. The idea was as much his as it was the commander's. Aminu had sought greater autonomy of the various ethnic groups of the north, the Middle Belt, and elsewhere, but his party was in a shambles on the eve of the civil war. Intimidation and violence had shattered the political machine he had built. The first coup had eliminated the Sardauna, Aminu Kano's opponent, and no new leader of the conservative forces emerged.

Aminu Kano is in the tradition of the mujāddidūn, reformers of Islamic society who have so effectively altered the course of western Sudanic history. The prototype in Nigeria is 'Uthmān dan Fodiye, whose reforms, backed with military force, created the Sokoto caliphate. Adeleye recounts the first century of the Sokoto presence: establishment of the caliphate, its constitution, internal rivalries, relations with other African polities, intrusion of Europeans, and British conquest. This story of massive dimensions is competently told; that this could be done is a mark of the maturity of historical research in Nigeria for Adeleye draws upon the works of other historians who have published in the last decade. About a decade after Adeleye leaves off his account Aminu Kano was born into the society that resulted from the adjustment of the caliphate to British rule.

The revolution that Aminu worked for was not just the removal of the British, but also of the arbitrary power of the emirs, the heirs of dan Fodiye and his followers who had become as oppressive as the authorities their predecessors had overthrown. This revolution has now

been accomplished to a great extent, but not in the way Aminu would have wished or could have foreseen. This modern reformer is unlike the founder of the caliphate in that he advocated nonviolent means. Dissatisfied majors set things off on the road to war by an uncompleted coup, and before it was over the emirs had been reduced in power and more contained in institutionalized restraints. While he was not personally the means of accomplishing his reforms, and is not by any means satisfied with how things are, he is a man whose life has been a forceful influence on his people. Aminu's life story exudes hope; the failure of the caliphate to attain and maintain its highest ideals and the bitterness of civil war reek of the tragic sense of life. That the wars are now in the past and the humanist reformer is still on the scene to make further contributions, and that he has countrymen of comparable outlook, bodes well for an emergence from military government to full political participation again—and on a basis that can avoid the divisive antagonism that has cost them so heavily.

These three books—only one by a professional historian—are not particularly related except that they are about Nigeria and published about the same time, but taken together, while they leave out much, they tell a large part of the modern history of one of the most important nations in Africa.

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ASIA AND THE EAST

JOHN W. DARDESS. *Conquerors and Confucians: Aspects of Political Change in Late Yüan China*. (Studies in Oriental Culture, number 9.) New York: Columbia University Press. 1973. Pp. 245. \$12.50.

This book looks at the history of the Yuan dynasty from the viewpoint of the occupying power, the Mongols, while most works have taken the Chinese viewpoint. Thus, much attention is given to loyalties to family and tribe and the problems that the Mongols faced when they ruled over the Chinese. Professor Dardess states that from 1260 on, the Mongol rulers became "Confucianized," meaning they accepted the principles of filial piety and ances-

tral loyalty that could be integrated into the Mongol value system, without accepting Sinification, which could have involved loss of Mongol language, ancestry, and caste.

The book starts with the anti-Confucian intermezzo of Bayan between 1328 and 1340 and his racist pro-Mongol policy, equating Confucianism with Chinese nationality. This policy worked against the supranational factional alignments that had dominated the earlier part of Mongol rule; it did not succeed because there was opposition against it from the side of the Mongols themselves. The period following Bayan's rule, from 1344 to the end of the dynasty in 1367, marked the end of a truly Mongolian regime in China and the Mongolian steppes, and the Mongol dynasty became a Chinese dynasty in which the 3 per cent Mongols by law demanded a quota of 50 per cent of all qualified degree holders and were sucked into the Chinese factional struggles. The Confucians were split between modernists (Wang An-shih group) and conservatives (Szu-ma Kuang group). Dardess thinks that the conservatives did not constitute a faction, but were tied together by individual moral exertion, not by self-interest (p. 164). They first consulted the local gentry and got their cooperation before they decided to propose action; their opponents were for determined, planned action. But if we hear that the conservatives were accused of venality (p. 166), we may suspect that they defended local interests, for instance, in their failure to check coastal piracy. So we may doubt whether they were kept in control by strong "institutional loyalties" and not by clique ties, especially as dismissal from office meant execution or death within a year.

Dardess shows that the breakdown of Mongol rule after 1344 was basically a consequence of the breakup of the federation of Mongol tribes and not a consequence of general decadence. When rebellions began to break out within the tribes, although Tohto could at first crush them, he was soon forced to give up hopes of decisive military action against rebels, resulting in the eventual loss of south and southeastern China. Regional interests of individual Mongol leaders and their tribal associates began to thrive and could no longer be controlled by Confucian ideology. The final victory of the rebels and the end of Mongol rule

was, thus, not so much due to a national upheaval against the foreign rulers, but due to a breakup of tribal loyalties within a federation of tribes that graded the tribes according to their situation at the creation of the Mongol federation. Tribal federations of this type are always in danger when the leader of the federation is weak, and the last Mongol emperors, indeed, were weak.

Dardess's book, well based upon Chinese, Mongol, and other documentary sources, will change many of our views concerning this period of central Asian or Chinese history. Some problems, in my opinion, still have to be discussed. For instance, because the Mongols officially sponsored Lamaism and Buddhism, to what degree was the Confucianism of the Mongols acceptable to Chinese? Does not the Mongol stress upon ethnicity indicate their ideological orientation? While we see that the conquerors leaned toward some form of Confucianism, or toward Confucians, we would like to see why they also appealed to currents in China that were opposed to Confucianism. Perhaps we also should know more about the background of the economic policy of the period between 1328 and 1367, which followed a period of strong financial and fiscal interest of Mongols in China. But the main value of this book is to take a solid look at the Mongol dynasty from the viewpoint of the conquerors and rulers. Much of what the author has said here can be used for a deeper understanding of other dynasties of nomadic conquest.

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PHILIP A. KUHN. *Rebellion and Its Enemies in Late Imperial China: Militarization and Social Structure, 1796-1864*. (Harvard East Asian Series 49.) Cambridge, Mass.: Harvard University Press. 1970. Pp. 254. \$8.50.

This work is a brilliant analytical study of the process of militarization that accompanied the breakdown of imperial control in the countryside during the late Ch'ing period. The first part of the study traces the historical development of various forms of militarization in traditional China, the uniquenesses of early Ch'ing forms, and finally the emergence of the gentry-

led *t'uan-lien* militia system in south and central China during the White Lotus Rebellion and its aftermath. This material is woven around the author's definition of militarization—a process by which men are severed, in varying degrees, from their roles in civilian life while bearing military responsibilities (pp. 13–15). In a superbly reasoned analysis the author demonstrates that bureaucratic divisions, such as *pao-chia* and *li-chia*, were interfused with “natural” organizational units in local society and thus easily molded into a cohesive hierarchy of *t'uan-lien* units.

The second part of the study deals with the Taiping Rebellion and the way in which the *t'uan-lien* system became the cornerstone of imperial defense. The author carefully analyzes the strengths of the personal armies that were built on the orthodox fabric of gentry dominated *t'uan-lien* units and contrasts them with heterodox alternatives, such as the Taipings. He concludes that the expansion of the *t'uan-lien* system during the Taiping Rebellion resulted in direct gentry control of subprefectural government in the postrebellion period, which, to some extent, was to shape political developments in twentieth-century China.

The contributions of this study are enormous. In addition to binding the Taiping period to the beginning of Ch'ing decline with the tough thematic threads of local militarization, the author has put historians on the scent of a whole range of problems regarding local control. Was the process of militarization a phenomenon unique to south and central China, or was it being generated throughout China by conditions of imperial decline? How important were gentry-led militia in controlling piracy on the China coast prior to the Opium War period? Was direct gentry domination at the local level always a function of the militarization process? How does one account for situations where heterodox forms of militarization seem to have persisted or were never completely enveloped by gentry organization? Does the direction of the militia movement in the early nineteenth century bear the unique imprint of the statecraft reformers, many of whom were Hunanese in origin? One hovers between the search for historical uniqueness on the one hand and generalization on the other. The

answers lie, as the author suggests, in further research into late Ch'ing local history.

Viewing the study more broadly, this book is a pivotal interpretive work that directs the scholar's attention to the internal dynamics of late Ch'ing history and away from the somewhat Europocentric concern with China's responses to external stimuli on the China coast. The author has sensitized late Ch'ing historians both to Peking perceptions of the nineteenth-century historical moment and also to hinterland responses to those problems that were, in Chinese eyes, the great historical issues of the times.

This work is a rigorous and creative piece of scholarship and unquestionably one of the most significant works to appear on late Ch'ing history.

JANE KATE LEONARD
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JEAN CHESNEAUX. *Peasant Revolts in China, 1840–1949*. Translated by C. A. CURWEN. (Library of World Civilization.) [New York:] W. W. Norton and Company. 1973. Pp. 180. \$7.95.

Most peasant movements are almost by definition local, sporadic, and fragmented. To assess their role in the history of China between 1840 and 1949 in 166 liberally illustrated pages is in a sense already a tour de force. This little book does more, however. For the nonspecialist it is a compact and clear introduction to the subject, while the specialist will find it interesting and useful as a general interpretation by one of the more eminent scholars in the field. The book is well written, and here credit should be given not only to Professor Chesneaux's original text, but also to the translator, who is himself a scholar in the same field.

The basic concern of the book is stated on page 20: “What historical role did peasant movements play in the evolution of imperial China through the ages? What function did they have in the dynamics of Chinese history?” The first four chapters deal mainly with peasant revolts in the nineteenth century, with an excellent introductory chapter that places these revolts against the premodern rebel tradition. The last four chapters deal with peasant movements in the twentieth century, but focus mainly on that part of the peasant movement

led by the Chinese Communist party, so that in effect the book shifts at midpoint from a study of peasant movements as a whole to a discussion of only that part of it which "accomplished the agrarian revolution." This is unfortunate because, as Chesneaux himself points out on page 83, probably more peasants were involved in "archaic forms of struggle" during 1911-49 than were involved in their "modern counterparts." The second half of the book is really a study of the peasant role in the Chinese revolution, and that is rather less than the title of the book promises.

The book also bears some traces of having been hastily done; there are various inconsistencies of some importance. For example, on page 62 Chesneaux refers to the Elder Brother Society's occupation of Sian in the far northwest in 1911, and five pages later categorically states that the society operated only in the Yangtze Valley. Similarly, there are inconsistencies (see pp. 65 and 72) in his assessment of the effects of Western imperialist pressure on peasant activity and in his evaluation of how "radical" the Peasant Associations of the 1920s were (pp. 90-95). In spite of these and some minor factual errors, this is an insightful and useful little book that should be particularly welcome to those whose field is not China but who would like, perhaps for teaching purposes, an introduction to some aspects of the Chinese historical experience.

ELLA S. LAFFEY
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PAUL RICHARD BOHR. *Famine in China and the Missionary: Timothy Richard as Relief Administrator and Advocate of National Reform, 1876-1884*. (Harvard East Asian Monographs, 48.) [Cambridge, Mass.:] East Asian Research Center, Harvard University; distrib. by Harvard University Press, Cambridge, Mass. 1972. Pp. xviii, 283. \$4.50.

Timothy Richard's empathy with the Chinese and his efforts on behalf of the reform and modernization of China make him one of the more attractive figures in the Western crusade to Christianize China. They also made him one of the more influential, but atypical of the nineteenth-century Protestant missionaries. Paul Bohr argues that Richard's experiences in the north China famine of 1876 to 1879 were the

source of his increasing concern with social, political, and economic reform; indeed, after his relief efforts in Shantung and Shansi, Richard turned to publicity among the literati to encourage far-reaching institutional change as the core of his evangelistic activities. Bohr says, furthermore, that the famine origins of Richard's reform program led to an emphasis on the welfare of the populace that distinguished his proposals from those of officials advocating reform because of military, economic, and ideological threats to the Middle Kingdom. Despite mission criticism of Richard's secularism, Richard found no conflict, for he believed that the advantage of Western civilization "over Chinese civilization was due to the fact that it sought to discover the workings of God in Nature, and to apply the laws of Nature for the service of Mankind" (p. 146). Richard never doubted that the Christianization of China was essential to the modernization of China even as it had been essential to the progress of the West; sympathetic as he was toward the Chinese people, his goal was to revolutionize and to destroy much of the Chinese heritage.

The scope of Bohr's work is limited, and it will alter no current interpretations of nineteenth-century China or the reform movements. Since he ends his study in 1884, Bohr does not examine Richard's influence on Liang Ch'i-chao, K'ang Yu-wei, and other reformers. Bohr summarizes rather than analyzes or assesses Richard's reform proposals. Richard's initial definition of his mission was a broad one, and his relief work seems to me to have contributed specificity to his reform program rather than to have been its origin.

Bohr, nevertheless, provides useful information and insights. His details on the devastating effect of the famine and the inadequacy of government aid vividly illustrate the reality of negative checks on population and the limitations on relief before modern technology. The Chinese response both during and after the famine stressed relief and rehabilitation in a static economy, and Bohr quite rightly contrasts this with Richard's postfamine search for means of prevention and reform. Even so, the magnitude of financial contributions by gentry and merchant as revealed by Bohr is impressive. However cumbersome and belated the government efforts at relief and however much they

were motivated by fear of rebellion, they dwarfed the efforts of Westerners. What set Richard apart as a relief administrator was his effort to develop a program ensuring equity and efficiency in distributing relief to individual sufferers. His unusual tact in working out such a program in cooperation with Chinese officials who were suspicious of his motives and who tolerated extensive inequities and profiteering help explain his later influence as reform advocate.

JESSIE G. LUTZ
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HAO CHANG. *Liang Ch'i-ch'ao and Intellectual Transition in China, 1890-1907*. (Harvard East Asian Series 64.) Cambridge, Mass.: Harvard University Press. 1971. Pp. 342. \$11.00.

Despite the sizable existing literature on Liang Ch'i-ch'ao, Dr. Chang's volume is a welcome addition, for it is a forthright exposition and analysis of Liang's thought during the most creative period of his life. Throughout the book the author carefully adheres to the substance of Liang's thinking and strives to pinpoint its meaning. His explorations of the antecedents and polarizations of Liang's ideas are unencumbered by tendentious hypotheses or contrived points of view. Dr. Chang's style of writing is serious and at times ponderous, but his interpretations are marked by accuracy and authenticity.

Dr. Chang's theme is that Liang Ch'i-ch'ao, whose writings were read by every educated Chinese at the turn of the century, opened the door to a new age by preaching the inadequacy of the old Confucian order, the need for building a modern state, the urgency of national cohesion and dynamic growth, and other related messages. This theme is, of course, well known to students of Chinese history. Dr. Chang's contribution lies in giving depth to the theme by a systematic and searching analysis of Liang's writings. Especially effective are the chapters on "The Foundation of Liang's Reformist Thought," "The New Citizen," and "The New Citizen and Private Morality," in which the concept of *ch'ün* ("the group"), the ideal of citizenship, and the importance of public morality, among other matters, are dis-

cussed with such fidelity of insight that few readers can fail to see why Liang's ideas were intimately linked to the rise of modern China.

With the advent of revolution and communism Liang's writings ceased to be popular reading. Yet his ideal of the "new citizen" contained elements of enduring validity that continued to inspire leaders after him, including the Communists. This persisting impact is a matter of considerable significance for understanding the Sinification of Marxism in present-day China. In Dr. Chang's study one can easily trace the genesis of such Sinification. Thus Liang's discourses on the imperatives of civic virtue, altruism and service, on the moral vice of selfishness, on the need for struggle and progress and for activism against fatalism, on the collective rights of the people, and above all, on the importance of existential decision, motivational commitment, and self-rectification as techniques for character cultivation—all these precepts dealt with extensively in this volume are among the spiritual roots of Mao Tse-tung's programs for national reconstruction. However unconsciously, Mao is a synthesizer of the process of Sinifying Marxism, which has long been in the making. The continuity of Chinese thought in spite of foreign ideologies is one aspect of Dr. Chang's study that no discerning reader will fail to grasp.

PING-CHIA KUO
Southern Illinois University

PAUL F. LANGER. *Communism in Japan: A Case of Political Naturalization*. (Hoover Institution Studies: 30. Comparative Communist Party Politics.) Stanford: Hoover Institution Press, Stanford University. 1972. Pp. xv, 112. \$5.95.

That American scholarship has long exercised a near monopoly of Western-language studies of communism in Japan will come as no surprise to readers familiar with the published research of Professors Robert Scalapino, Hyman Kublin, George Beckmann, Hans Baerwald, and others. It is perhaps fitting, therefore, that Paul Langer, whose pioneer *Red Flag in Japan* (1952, coauthored by Rodger Swearingen) did much to establish this peculiar, Perry-like tradition, should now offer the first monograph-length analysis of Japanese communism aimed at that protean soul, the nonspecialist (that is, general) reader.

This timely study, part of a series on non-ruling Communist parties, has three instantly appealing virtues: it is concise, clear, and reasonably up to date (including footnote references to the Eleventh Party Congress of 1970). With an expert's sure grip, Langer has managed to squeeze an impressive amount of information into seven slim chapters. He skillfully surveys the checkered fortunes of the Japanese Communist party from its origins to the present, giving decided emphasis to the party's postwar efforts to integrate itself into the changing realities of, in Scalapino's words, Japan's "post-Marxist society." The author contributes little that is new and advances no grand thesis other than the common sense one that the Japanese Communist party is gradually being forced to become "naturalized" to its environment. There is also a disturbing degree of repetition for a mere ninety-seven pages of text, though in part this may have been imposed on the author by the prescriptive format of the series (described in Jan Triska's introductory essay in suitably hyphenated and semicomprehensible "behavioralese"). Some fleshing out, finally, would have done wonders for Langer's purpose, such as adding two or three brief but representative biographies illustrating why some ten per cent of the Japanese electorate has now been drawn to communism. But these are annoyances rather than fundamental defects and should not significantly detract from the welcome this handy book will receive in many quarters. College students with awkward term papers, in particular, will likely pay Langer the ultimate campus honor of cribbing his material for years to come.

ROBERT JOSEPH GOWEN
East Carolina University

BAIJNATH SHARMA. *Harṣa and His Times*. With a foreword by RAJ BALI PANDEY. Varanasi: Sushma Prakashan 1970. Pp. xxxii, 527. Rs. 75.

Dr. Sharma has put a great deal of labor into his book. Its value is basically that it is a compilation of all that is known about Harṣa and his times. Interesting headings do not always meet the expectations aroused, but the author has been conscientious in the collection of his material, though one doubts if he has been able to check the Chinese of Hsuan Tsang. He

is very respectful of other scholars, and the style is perfectly readable. For those scholars or libraries that do not have monographs on Harṣa the book can be fully recommended.

My real criticism of the book is that it is not better than previous works. The author's historical reconstructions are not correct any more frequently than those of his predecessors. There is also what might be called a pro-Harṣa chauvinism, but as his antagonists were also Indian this is not necessary, and it denigrates many for the sake of one. One would have liked to see Harṣa's policies interpreted in light of the Kautalyan mandala, such as his alliance with Bhaskaravarman of Assam. The reader will see in the first two chapters the extraordinary difficulty of producing order in that jungle of acts known as early Indian political history. It needs originality of approach as well as labor to make real progress. It would be most useful if Dr. Sharma were to collect and annotate all contemporary inscriptions, say from A.D. 600 to A.D. 650, a task for which he is well qualified, thereby clarifying evidence and extrapolation.

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K. ISHWARAN, edited with an introduction by. *Change and Continuity in India's Villages*. (Southern Asian Institute Series, Columbia University.) New York: Columbia University Press. 1970. Pp. 296. \$11.00.

DAVID G. MANDELBAUM. *Society in India*. Volume 1, *Continuity and Change*; volume 2, *Change and Continuity*. Berkeley and Los Angeles: University of California Press. 1970. Pp. xi, 323, 37, 14; ix, 328-665, 37, 14. \$10.00 each.

Recent years have produced a spate of Indian village studies. The Ishwaran essays are a collection of ten such papers with an exemplary prologue by S. N. Eisenstadt. All of the other essays describing investigations of particular villages scattered across the face of the subcontinent are first-rate. We are indebted to William H. Newell, Alan R. Beals, Gerald D. Berreman, Joseph W. Elder, Kathleen Gough, K. Ishwaran, Joan P. Mencher, Henry Orenstein, Yogendra Singh, and Stephen Tyler for their illuminating contributions. The overriding consideration of this anthology is to establish the patterns of social change in traditional and modern India.

It is apparent that the Indianess of Indian culture stems from the Brahmanic system of values. One aspect of this traditional culture is the autonomous character of the entire network of castes and villages. Theoretically standard jatis existed on a country-wide basis. This in fact was not the case. Probably the census inaugurated by the British gave the greatest impetus to the establishment of a unified social hierarchy. (This same point has been made by Thomas Metcalf in another place.)

The new pattern of change, which has been called "modernization" as opposed to "Sanskritization," advanced along various lines: economic, professional, and political. The British, of course, introduced a new polity when they created political centers, and the All-India Congress then shaped this ideology into a broader political unity.

Such developments provide the underpinning for a new type of change in Indian history. The new order was relatively autonomous in relation to the old, but it generated cultural and social change of a most significant kind. Indeed the caste order itself changed from a sectarian to an associational one. Even if the protest of regional and linguistic areas seemed to be traditional such protest was phrased in political and secular terms.

David Mandelbaum's comprehensive work stems out of his doctoral research of 1937, ideas steeped in years of field work and honed by discussions with colleagues. He deals with the same central problem that is presented in the Ishwaran essays. His first volume includes sections on "Family and Kinship Relations," "Relations among Peoples of Different Jatis," and "Relations with the Jati." A similar organization in the second volume provides sections on "Village, Region, Civilization," "Recurrent Change Through Social Mobility," "Recurrent Change Through Religious and Tribal Movements," and "Continuities and Trends."

Mandelbaum indicates certain contradictions in contemporary Indian society. For example, although the gross national product has risen steadily India has had to run hard to stay in the same place because of the absolute increase of population. Improved health measures, although they lengthen life, exacerbate the population problem. And the distribution of land, goal of the egalitarians, may in the beginning

lower production because of the lack of experience of the small holder.

The author is quite critical of Gunnar Myrdal's picture of the apathetic Indian villager. Village studies, he finds, show that the desire for status is a constant incentive to hard work. Such studies also indicate that it is not lack of initiative or the suffocating nature of caste that weighs down society. The main problem is not how to eliminate caste, but how to use productively the traditional forces of village society. Villages usually adapt traditional standards to embrace new roles and institutions.

One of the best indications of the improved position of women is the fact that widows are now allowed to remarry. The seclusion of women is less evident than formerly. The education of girls has been increasing rapidly, and many women are now employed in clerical, administrative, or professional work. These changes were probably hastened by the change in attitude of young men as they gained broader experience.

Elaborate weddings and large dowries are still common, but they serve the function of increasing social status, and even financial credit. Some marriages occur between members of different castes, but such unions are not common even among the urban and educated. Even so there is evidence that the social distance between castes has lessened. Some Harijans have become Buddhists to escape caste restrictions.

Many of the same devices seen elsewhere enable Indian politicians to manipulate votes, but a large jati cannot be ignored. Caste membership has become significant in wide political arenas, and provincial or national contests often display the same characteristics that are seen in village elections. The national politicians behave in the same manner as village politicians not because they are only the village elders translated to a higher position, but because the new political elite originates in the same civilization as the villages and holds the same values.

Mandelbaum has written a work that will become a classic, and students of modern India will overlook it at their peril.

MARK NAIDIS

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WAVELL. *The Viceroy's Journal*. Edited by PENDEREL MOON. New York: Oxford University Press. 1973. Pp. xvi, 528. \$25.75.

This handsome book is the most detailed personal account that we have of a British viceroyalty in India. It provides a minute chronicle of political events in India from October 1943 to March 1947. But two caveats need to be given. The editor, Penderel Moon, has subjected the original diary to "substantial cuts." Second, Wavell wrote this diary with one eye on posterity, and thus it needs to be read as a personal apologia.

The historian of the period will find no substantial revelations here, but there is new detail on many points. Examples are Wavell's early advocacy of a firm and public date for the end of British rule in India and the extra detail on his breakdown plan. More generally, the whole book is a case study of a Viceroy's difficulties with the cabinet in London. Churchill and Attlee both disagreed with Wavell on how to govern India, and when conflicting personalities are added it becomes remarkable that Wavell stayed in office as long as he did.

The main value of this book lies in what it reveals of Wavell as a man and as a Viceroy. He emerges as a most estimable member of the British upper middle classes—literate, humane, stolid, hardworking. He was ideally suited to be a troop commander, which is what he had been, or a minor country gentleman, but as Viceroy he was fundamentally out of place in a period when India was clearly, though with difficulty, moving toward independence.

Part of the trouble was that Wavell had too much stiff integrity and too little public charm. Mountbatten's character was the reverse. The job required a little of the "slipperiness" that Wavell so detested in politicians, and that he himself lacked. More basically, Wavell remained a soldier and an imperialist, a man lacking the ability or the imagination to approach the leaders of the national movement with any empathy. His strictures on Gandhi are sure to be widely quoted ("an unscrupulous old hypocrite," p. 353), but other attitudes are more truly revealing. Wavell was able to claim that the British had tried their best to bring Hindus and Muslims together (p. 193).

After the war the British used Indian troops to try and put down the nationalist movements in Burma and Indonesia; Wavell was unable to see why Nehru and others got upset about this. (Nor, it is interesting, can his editor, Moon.) In 1944 Wavell found that the British "were a very great nation, greater than the American, and would remain so" (p. 76). Like many other Englishmen, Wavell found the princes compatible and characterized their myopic demands as showing "moderation and realism" (p. 311). Despite the long history of tension between India and South Africa, he took an appointment as a director of de Beers soon after his retirement as Viceroy. Perhaps most indicative of all, in all the evaluations of personalities in this book, the only people praised without reservations are British, with the exception of Casey of Australia, who presumably was given honorary status as a "man" who "faced his fences."

The point is not that Wavell was a racist, for Americans and the London politicians are vigorously condemned. Rather, he simply lacked the ability to cut through the tantrums, the interminable bickering, the rudeness of Indian politicians and realize that beneath this these men were, in their own terms and toward their own country, as patriotic as he was. He was unable to understand the suspicions of British duplicity that, with some reason, most Indian politicians by now harbored. Most basically, Wavell was simply unable to appreciate the force of Asian nationalism in his time: as a result he was ineffective when he was placed in the middle of the maelstrom.

M. N. PEARSON

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ROUNAQ JAHAN. *Pakistan: Failure in National Integration*. New York: Columbia University Press. 1972. Pp. ix, 248. \$12.50.

Rounaq Jahan's book is one that can be read with profit by specialists of several kinds. It is, first and most importantly, a meticulous examination of the efforts at national integration introduced during the Ayub regime (1958–69). That these efforts failed miserably in East Bengal is a thesis carefully documented by the author and underlined by history—the emer-

gence of Bangladesh in December of 1971. Second, the data on Pakistan are presented within the context of the literature on political development; one can review the "Pakistan case" in the language of the theorists. It is regrettable that, since general developmental theory is not very refined, the specific Pakistani material does not fit perfectly. The book is, finally, a splendid background to the formation of Bangladesh. For each of these themes—national integration, political development, and the emergence of Bangladesh—there will be further information and interpretations in new works, but Rounaq Jahan's seminal contribution is bound to be a major source for a long time to come.

The book includes a brief overview of the East-West Pakistan imbalances in the 1947–58 period. It then systematically analyzes the Ayub regime's integration policies in economic development, in the bureaucracy, and in local and party government, especially as these policies related to East Pakistan. It is not, of course, a balanced narrative on Pakistan. The thesis shows the failure of the central government of Pakistan to integrate its largest human sector, East Bengal, into the Pakistani nation. Rounaq Jahan is well aware that there were other objectives, such as state-building and economic development, but her thesis is that there was no more crucial objective than national integration and that if it failed, all else would. One used to argue about this, but Bangladesh settled the matter.

The quality of the author's analysis that has its most impressive impact is her evaluation of the role of the Bengali counterelite, more particularly the vernacular elite, in response to thrusts for change introduced by Ayub. My interpretation is that no matter what was done, it was either wrong or not enough. If the emphasis at one time was economic growth, then the counterelite demanded something else instead. If there was East-West parity at a particular bureaucratic level, then the "power differential," in fact, was claimed to be skewed to the West.

Rounaq Jahan's study concludes that Pakistan as a unity failed in national integration. That seems true enough. Her book is for me also good evidence (from both West and East) that it could never have succeeded; indeed, East

Bengal as a part of Pakistan was not a problem but a mistake.

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UNITED STATES

STOW PERSONS. *The Decline of American Gentility*. New York: Columbia University Press. 1973. Pp. viii, 336. \$11.95.

In recent years the quantitative study of various elite groups has become fashionable. Stow Persons has now written a more subjective, impressionistic account of one particular group, what he calls the nineteenth-century gentry elite, and its decline, interpreted largely in terms of modern mass theory and based on a limited selection of contemporary writings and secondary works that almost certainly would not satisfy the statistical-minded. Probably traditionalists will criticize him for adopting an approach that they have not yet accepted themselves, while the avant-garde will carp at the way he has done it.

Persons argues that the American Revolution destroyed the colonial gentry ruling class and that in its place there arose several different functional elites—including the gentry elite, which was composed largely of professional people—that often overlapped with the socio-economic elite in a happy combination of wealth and cultivation. Most of the examples he gives of this gentry elite come from New England, some from New York, and almost none from as far south as Philadelphia; it was a phenomenon apparently unknown in the West.

The gentility of manners of this group was joined with high cultural activity, so that the gentleman became the apostle of culture with a mission to elevate the national mind in a society marked by democratic capitalistic mobility. There would be, it was assumed, respect and status for the natural gentleman who represented an aristocracy of virtue and talent and who was often thought to have emerged out of the humble yeomanry.

Readers who prefer specific illustrations to abstract theory will find the most interesting sections to be the discussions of the gentry lady and her partial emancipation en route to

becoming the New Woman, the role of clubs for gentry social life, the rise of various academies to combat mass culture, and the impact of *The Nation* as the inculcator of correct political and cultural attitudes, as well as the excellent analysis of how the novels of William Dean Howells document the social mobility, tensions, and gentry attitudes of the day. Persons also points out that the gentry remained active in political life much longer than has been thought, even in the administrations of such implausible patrons as Jackson and Grant.

The second half of the book is the melancholy chronicle of the decline of this gentry elite as undermined by mass culture. The late nineteenth-century reform efforts of the Liberal Republicans and Mugwumps proved increasingly ineffective until the gentry turned from politics to become instead the experts who had the scientific efficiency for running the welfare state. In the universities the "moderns," such as Charles W. Eliot, hoping through greater educational freedom to produce such expert gentry leaders for a democracy, successfully undermined the efforts of the "ancients," as represented by Noah Porter of Yale, to preserve the classics as the best way to train gentlemen. With increasing pessimism Henry Adams and Charles Eliot Norton recorded the effects of mass society upon culture, documenting the predictions of Tocqueville half a century earlier and anticipating the twentieth century's alienated intellectual. Finally, there are brief summaries, perhaps not too well integrated with the preceding narrative, of modern mass theory from Josiah Royce to Philip Selznick, much of which will be unfamiliar to the traditional historian, if, indeed, there are any left.

Professor Persons, himself a distinguished example of what a New England clerical and Ivy League background can still produce, makes no effort for detailed comprehensiveness but instead has written a series of related essays, sometimes a little disjointed in effect, but often stimulating in their insights. The style is tight, dry, and abstract, seldom relieved by wit or frivolity, but always literate, though not even an Old Blue like Stow Persons has figured out how to present sociological theory with flair. And throughout there is a wistful elegiac tone

to this sad story of the departed glories and no longer relevant values of a social class.

WALLACE EVAN DAVIES
University of Pennsylvania

RICHARD W. VAN ALSTYNE. *The United States and East Asia*. (Library of World Civilization.) New York: W. W. Norton and Company. 1973. Pp. 180. \$7.95.

In his epilogue Professor Van Alstyne quotes approvingly Hegel's remark that "peoples and governments have never learned anything from history." Yet he must have some hope that his strong revisionist statement of America's role in East Asia will do something to change the way in which that role is played in the future. He has written a brief, general work that incorporates some insights of recent scholarship. It will not tell Asian scholars anything they do not know, but it will give the general reader a sense of the whole spectrum of the American-East Asian relationship and will leave him with an illuminating if disturbing awareness of the disparity between wish and reality in the conduct of that relationship.

Professor Van Alstyne starts, sensibly, not with the *Empress of China* but with a chapter on China's traditional mode of dealing with frontier barbarians. Against this background he then discusses the seaborne Western intrusion, including the United States, and displays the problems presented by the West as something new that Chinese statesmanship, in one of its less creative periods, sought unsuccessfully to meet through repetition of the old formulas. In contrast, Japan's long isolation meant that the American incursion was more of a shock. But it also meant that, once isolation was broken down, the Japanese were not burdened by traditional policies and could deal with the Western barbarians with a realism which long eluded the Chinese. As a sort of coda to these opening chapters, the author discusses the rise of Chinese antiforeignism and of American prejudice against the Chinese, both bitter fruits of mutual misapprehension.

From that point on, the last century in East Asia is a series of variations on the themes of China's disintegration, Japan's rise as an Asian power, and European and American imperialism, ending with the emergence of the Com-

munist regime in China. Here the author is at his best, especially in depicting that sometimes incredible blend of good intentions, opportunism, and myopia through which Americans have, in Professor Van Alstyne's view, led themselves to repeated disasters in the Far East. He overstates his case and really does not give the "recent scholarship," which might add balance to his picture, a fair show. But he is never bland, and between his strong views and a lot of good, well placed illustrations, this is a lively and useful volume.

EDWARD D. GRAHAM
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WARREN I. COHEN. *America's Response to China: An Interpretative History of Sino-American Relations.* (America and the World.) New York: John Wiley and Sons. 1971. Pp. xii, 242. Cloth \$7.95, paper \$3.95.

FOSTER RHEA DULLES. *American Policy toward Communist China, 1949-1969.* Foreword by JOHN K. FAIRBANK. New York: Thomas Y. Crowell Company. 1972. Pp. xiii, 273. \$7.95.

Neither Warren I. Cohen nor the late Foster Rhea Dulles conceived of these books in terms of primary research. Cohen's *America's Response to China* is part of the ten-volume *America and the World* series, which has been designed for undergraduate reading. Dulles has summarized American policies and attitudes, drawing upon a variety of printed sources. In a word, both volumes are to be regarded as syntheses, not as works contributing new knowledge.

America's Response to China is a small book, which, nevertheless, manages to sweep the record of Sino-American relations, 1844 to the late 1960s. Like companion volumes in the larger series, it sticks closely to the story of a bilateral relationship. As a result this is a volume offering a decidedly limited topical coverage. Professor Cohen was precluded from saying much about the Japanese or British impact on American policy. He was similarly limited in discussing the origins of American policy. This is a case of a writer's being cramped by a rigid format. What the general editor attempted was a series that could serve varying purposes in undergraduate instruction. Used individually, these volumes might supplement a basic text, or, when purchased in varying

combinations, they might provide basic reading. The series, however, has been built at the expense of its components. When viewed against the possibilities opened by scholarly research, the substance of this little book seems rather thin.

This is not to say that Professor Cohen is to be faulted for his handling of his assignment. He has met the limitations of format by outlining a few basic themes in a remarkably sure-handed way. He presents the United States as having pursued in Asia (as in Europe and elsewhere) a world in which its material interests and ideals could thrive. Translated into policy, these objectives led to American support (until 1949) of China's administrative and territorial integrity. Yet, since China was distant and tangible American interests there were few, American officialdom was unwilling to risk much in the pursuit of these objectives. The result was a policy characterized by ends that generally were beyond the reach of available means. It was a policy that encouraged Americans to deceive themselves: Senator Kenneth Wherry is puckishly quoted as assuring some constituents that "with God's help" they could "lift Shanghai up and up, ever up, until it is just like Kansas City." But, unlike some other writers, Professor Cohen is not inclined to see any great harm arising from this exaggerated self-esteem. On the whole Americans were ready to accept much less than they hoped for in China. As for China herself, she got very little out of the Americans, but neither did she especially suffer from American conduct. This view, for example, emerges in a summation of Franklin Roosevelt's policy: "Once again, the Roosevelt years demonstrated that American policy was designed to serve American interests without any particular regard for China. The fact that China too benefitted was as incidental as it was undeniable. Roosevelt's East Asian policies gave Americans *no* cause for grievance—and the Chinese *no* cause for gratitude." It is only when the narrative reaches the years after 1949 that Professor Cohen begins to criticize. American efforts to "contain" the People's Republic reflected an official concern with China that was all out of proportion with this nation's tradition. Moreover, containment set the United States for the first time in opposition to the mainstream of China's modern development.

Containment thus appears as a "great aberration" in United States policy that has proven harmful to Americans and Asians alike.

Professor Dulles assumes much the same stance, but his book has been organized on a vastly different scale. By writing at greater length and focusing on the cold-war years alone, Dulles has managed to describe the processes of policy formation. His history opens with an account of the uncertainties that confronted Harry Truman when Mao Tse-tung conquered China: in 1949-50 the United States was still bound by legacies bequeathed by its support of the Kuomintang-Nationalist government, but it had begun a search for an accommodation with China's new rulers. America's failure to remain on this path is explained in terms of American politics and Communist ambitions. No other writer has portrayed more vividly the interplay of personalities, ideas, and partisan rivalries that constituted the domestic basis of this nation's China policy: Dean Acheson, a brilliant and able administrator who was flawed in his dealings with Congress; Senator Joseph McCarthy, a marauding and unprincipled opportunist who not only gutted the State Department but who also limited the nation's intellectual responses to China's revolution; and John Foster Dulles (a kinsman for whom the author exhibits little compassion), who envisioned American restraint of China as a kind of holy crusade. The foreign origins of Sino-American difficulties are depicted in more prosaic terms. Nevertheless Dulles makes abundantly clear his conviction that the cold war was not made in the United States alone. If American officials were dead wrong in assuming that Communist China was a Soviet puppet, they are not charged with errors in estimating Russian ambitions. Dulles views the Soviet's role in instigating a Korean conflict as only a single instance of that nation's reaching too far. Nor has the People's Republic, a government that has proven remarkably obdurate and inflexible, made the settlement of Asian problems any easier.

American Policy toward Communist China was completed just prior to the author's death in the fall of 1970. The book demonstrates that to the very end of his life Professor Dulles retained the powers which made him one of the nation's most widely read historians. In Dulles's

hands the record of Sino-American relations becomes a story that offers lessons; his characters live, behaving wisely or foolishly; and we are vividly confronted with the dilemmas and uncertainties of those who have gone before. During his time, Professor Dulles understood better than most the importance of narrative. It seems especially fitting, therefore, that Dulles's last book should be coupled in a review with one by Professor Cohen, a younger man who exhibits much of the same understanding.

BURTON F. BEERS

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HILARY CONROY and T. SCOTT MIYAKAWA, editors. *East Across the Pacific: Historical & Sociological Studies of Japanese Immigration & Assimilation*. Santa Barbara, Calif.: American Bibliographical Center, Clio Press. 1972. Pp. xvii, 322. Cloth \$15.00, paper \$5.75.

The tone of this volume is set by the editors in a foreword that emphasizes the success of Japanese-Americans in assimilating with the majority society. They point out that despite hypotheses based on the experiences of European migrants, the Japanese-Americans, with greater cultural and ethnic differences to overcome, experienced less familial disorganization and juvenile delinquency than did most national-origin groups from Christian Europe. More than this, despite the preachings of organized anti-Oriental factions, the Japanese showed swifter and more marked success in integration with middle-class social values as represented by educational and professional standards. It seems to be tacitly assumed that this is good.

Inevitably, in a work of this sort, the papers vary in both type and quality. The first two sections deal with aspects of the history of Japanese out-migration, from the 1868 *Gannen-mono*, who journeyed to Hawaii, to a remarkably dispassionate personal narrative of the World War II relocation experience, as seen by a sympathetic Quaker social worker. David Purcell's interesting paper on "Japanese Entrepreneurs in the Mariana, Marshall, & Caroline Islands" seems strangely out of place here, having no apparent relation to the themes stressed in the rest of the book. Howard H. Sugimoto's bibliographic essay on Japanese relocation and its impact, on the other hand, de-

serves better than its place as an appendage to Esther Rhoads's account of her experiences with relocation and its victims.

Part three, entitled "From History to Sociology," seems to involve a misnomer. T. Scott Miyakawa's treatment of "Early New York Issei Founders of Japanese-American Trade" seems a useful but pedestrian sort of historiography, with little sociological method involved. Sharlie C. Ushioda is scarcely closer to sociology in examining the "value system" of Inazo Nitobe.

Among the sociological essays is the finest example of a mixture of history and sociology, S. Frank Miyamoto's perceptive account of Seattle's Japanese-American community. The paper that follows, concerning accident-proneness among children of Oriental ancestry in the Oakland, California, area, is much more method than matter.

The general effect of the thirteen contributions included in this volume does in fact support the hypothesis advanced by editors Conroy and Miyakawa. The Japanese in America belie the facile stereotypes of both friend and foe. Their successes in the American style have not been bought at the expense of complete destruction of ethnic identity. Both Japanese and American social ethics have guided this minority group along a path toward something more than passive "assimilation." Even third and fourth generation critics of Nisei compromises reveal rather than deny that fact.

DONALD D. JOHNSON
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WAYNE E. FULLER. *The American Mail: Enlarger of the Common Life*. (The Chicago History of American Civilization.) Chicago: University of Chicago Press. 1972. Pp. xi, 378. \$8.95.

The mail we receive is usually taken as much for granted as the air we breathe. Mr. Fuller's history of the American postal service therefore is a welcome reminder of the central role the mail has played in our social and communications systems. The volume takes its place among the topical group included in the Chicago History of American Civilization series, along with those by Stover on American railroads, Rac on the automobile, and Condit on American building.

Precisely because the mail has been so all pervasive and influential its impact is perhaps more difficult to specify than is that of other transportation or communications media whose consequences have been more dramatically obvious. Mr. Fuller makes a convincing case, which should not be—but probably is—needed, for one of the implications of his subtitle: that the mails have been deeply involved in many if not most of the major social movements from pre-Revolutionary days to the present. To me he is slightly less convincing in respect to the second implication: that the mail served as a causal force for social change and not just as a passive responder to pressures generated elsewhere. The latter case is strongest for the early years of the nineteenth century when congressmen and others, eager to encourage Western settlement, often pressed the mails ahead of the migrating population they were intended to serve. Even here, however, it is difficult to sustain the argument. Consequently the author does not push his case very hard.

After an opening review of the development of mail service in Europe and colonial America there follows a series of chapters detailing the developing postal system's involvement in popular demands for service, in bridging as well as in aggravating the sectional conflict that spanned the middle years of the nineteenth century, in popularizing and diffusing information, in serving and on occasion rivaling the operations of private enterprise, in fostering economic and political expansion overseas as well as at home, in registering as well as censoring changes in the nation's moral standards, and in responding to the continuous pressures generated by the American political and party systems. In most instances the chapters tend to bring the story to an uneven end, roughly around World War I, with occasional references to developments in later periods. A brief epilogue (eleven pages) attempts to reunite the several themes by sketching the problems of the post office in the years following World War II as background for the 1970 law that replaced the traditional system with a government corporation.

Both the advantages and disadvantages of a topical approach are well known and well illustrated here. Individual chapters document

effectively the deep involvement of the mails in the development of their respective subject matters. The necessity of repeating chronology in each chapter makes for somewhat weary and repetitive reading, although the author does succeed in lightening the reader's burden with occasional felicitous perceptions or turns of phrase. On balance this is a valuable reference book and a useful summary of the role played by an important, virtually unknown, institution. Intended for the general reader the book could well serve as an invitation to more detailed studies and fuller analysis of the growth of America's communications systems.

MORRELL HEALD

Case Western Reserve University

FRANK RICHARD PRASSEL. *The Western Peace Officer: A Legacy of Law and Order*. Norman: University of Oklahoma Press. 1972. Pp. xii, 330. \$8.95.

For this discourse on the Western lawman, Frank Richard Prassel throws a very large noose. He brings in marshalls, sheriffs, rangers, chiefs of police, mounties and rurales from across the borders, dog soldiers of the Plains tribes, customs inspectors, postal guards, park rangers, the Secret Service, and the U.S. Bureau of Investigation hunting down draft dodgers, Wobblies, and subversives during and after World War I. Also included are the private detectives and guards employed by railroads, banks, mines, merchants, manufacturers, and cattlemen's associations; posses and deputies; and those who rode shotgun for the express companies. Today's highway patrolmen also qualify as do lawmen of the type who carried out the 1965 shootup of the Muslim mosque in Los Angeles and the synchronized raids on the Black Panther quarters in 1969.

As befits a professor of political science or, as he puts it, the "science of criminalistics" (p. 111), Prassel's approach is analytical. His subject bristles with anecdotes and he does not resist them, but his prime purpose is to assess the role, routines, work habits, and contributions of the men who wore the badge. Because they constitute such a miscellany, generalization or even a clear prototype is difficult to construct. In the end the crux of Prassel's argument is that these lawmen were working

much as their counterparts do today, facing no more crime and using little more violence.

Folksay, he admits, runs to the contrary, and, like President Nixon, he puts the blame on the media and particularly television (also the movies, which apparently Nixon does not watch). Prassel points out that the novelists such as Wister, Rhodes, and Clark have not played up the Western peace officers as trigger-happy gunslingers. Early journalists did to some extent, but it was left for "Gunsmoke" to give this myth its ultimate currency. He insists too, though without explicit analysis of recent historical writings to prove it, that we historians of the West have also been infected by the myth of law and order achieved by right prevailing in those duels in the sun. His book will not stop the reruns of "Gunsmoke," *High Noon*, or *True Grit*, but it has much food for thought and is a useful counterbalance to runaway writing on the lawmen of the West.

JOHN CAUGHEY

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THOMAS HARRISON BAKER. *The Memphis Commercial Appeal: The History of a Southern Newspaper*. Baton Rouge: Louisiana State University Press. 1971. Pp. 336. \$12.50.

JAMES PLAYSTED WOOD. *The Curtis Magazines*. New York: Ronald Press Company. 1971. Pp. ix, 297. \$7.00.

Writing the history of a journalistic enterprise is not an easy task. On the one hand, one must resist the temptation to string together snippets and summaries of editorial opinion and then to offer them up as the history of a newspaper or magazine. On the other, too much concern with the internal machinations of ownership, finance, and managerial politics will tend to make the history little different from that of any other business enterprise. Of these two books, Thomas Harrison Baker's study of the *Memphis Commercial Appeal* occasionally veers too much in the first direction, while James Playsted Wood's history of the *Curtis Magazines* leans too much toward the second.

Founded as a Jacksonian Democratic news-

paper in predominantly Whig Tennessee, the *Memphis Appeal* (it did not become the *Commercial Appeal* until 1894) vibrated from a states'-rights position to one of moderation and sectional conciliation, and then back to support of secession on the eve of Lincoln's inaugural. In 1862, forced to leave Memphis ahead of the Union Army, it began a vagabond existence that carried it to many Confederate cities before its return home in 1865. The Reconstruction years saw a predictable stance in favor of white supremacy and the Democratic party, positions it did not abandon for more than sixty years. After 1894 the *Commercial Appeal* became a staunch opponent of free silver, an equally fervid supporter of U.S. imperialism, and, under the editorship of C. J. P. Mooney (1908–26), an ardent booster of Woodrow Wilson's foreign and domestic policies.

It was during Mooney's editorship that the *Commercial Appeal* took on Boss Ed Crump and the seemingly indestructible political machine that ran Memphis and sometimes Tennessee for nearly forty years. It was a losing battle, and eventually the paper backed off. (In retrospect, Crump's major offenses seem to have been his encouragement of black voters and his support of publicly-owned utilities.) In 1936 the *Commercial Appeal* was bought by the Scripps-Howard chain, and here Baker seems to lose interest in his subject, devoting only eight pages to the years since 1941. No explanation is offered, and the reader is left wondering about the role of the *Commercial Appeal* in the postwar period that saw so much Southern political and social history being made.

It is unclear why the *Commercial Appeal* is worthy of attention. With one or two exceptions, it never challenged any basic Southern belief nor did it speak for any particularly discrete constituency. It was not comparable in its impact with the newspapers of New Orleans, Atlanta, Richmond, or Louisville. With the possible exception of the Mooney years, it never represented the personal journalism of a Watterson, a Grady, or a Daniels. Whatever impact the *Commercial Appeal* did have is often obscured by Baker's inclusion of much editorial trivia in many chapters, though there are excellent summaries of the changing na-

ture of newspaper journalism and how the *Commercial Appeal* responded to them.

If Baker has possibly overestimated his subject, the same cannot be said for Wood's 290-page study of the Curtis magazines and the publishing empire associated with them. No one interested in the shaping of middle-class America in the first half of this century can ignore the importance of the *Ladies' Home Journal* or the *Saturday Evening Post*. Not only did they lead the field in circulation and advertising, but they pioneered in the areas of motivational research and consumer preference surveys, now accepted institutions in the advertising world. Wood, himself a former Curtis employee, makes clear his admiration for Cyrus H. K. Curtis, Edward W. Bok, and George Horace Lorimer, who founded and created the Curtis empire and bequeathed it to—in Wood's opinion—lesser and shakier hands in the 1930s. Wood draws upon his inside knowledge of Curtis history to describe the blunders and miscalculations of the last ten years of the original company, though the alternatives are never made clear. He is critical of the company's refusal to diversify into other media, yet such diversification by the rival Luce and Cowles empires did not save *Life* or *Look* any more than it would have saved the old *Saturday Evening Post*. The age of the mass-reader-ship feature magazine, thick with advertising and full of reassuring pats on the back to Middle America, could not have lasted forever in the face of competition from the electronic media. As in most cases, however, what doomed the old *Post* and *Journal* was not declining circulation but a lack of faith among its advertisers. The collapse of the Curtis empire is all the more stunning when one reads of the days in the 1920s when advertisers stood in line for the opportunity to display their wares in either the *Post* or the *Journal*.

Unfortunately, Wood's book is marred by what is usually referred to as a "racy" journalistic style of writing that does not measure up to the importance of his subject. The comings and goings of obscure managing editors clutter up the pages unnecessarily. Documentation is sparse. Whereas one of the weaknesses of Baker's study of the *Commercial Appeal* is his steady diet of editorial summaries and quotations, we are told little of the edi-

torial stance of the Curtis magazines except that it was conservative and grew increasingly anachronistic. We would like to know more about the impact of the *Ladies' Home Journal* upon American women and their self-image, but Wood passes up this opportunity as well. And finally, though he is an admirer of the Social Darwinism that typified so many businessmen of Curtis's and Bok's era, Wood omits notice of the irony of the Curtis empire being done in at last by the very values it tried to cement into the American mind: balanced budgets, mass advertising, and competitive journalism.

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ROBERT A. RUTLAND. *The Newsmongers: Journalism in the Life of the Nation, 1690-1972*. (Two Centuries of American Life: A Bicentennial History.) New York: Dial Press. 1973. Pp. xv, 430. \$12.50.

Robert A. Rutland has written a good and useful survey history of American journalism in the tradition of Frederic Hudson, Willard G. Bleyer, Frank Luther Mott, and James Melvin Lee. Former reporter, professor of journalism and of history, and now editor of the *James Madison Papers*, Rutland covers the subject well, considering the definition of journalism he set for himself: the "spoken, printed or visual report of timely interest to a mass audience." All the giants are here—from Benjamin Franklin, Hezekiah Niles, and Benjamin Day to James Gordon Bennett, Horace Greeley, Harry J. Raymond, Joseph Pulitzer, William Randolph Hearst, and Joseph Medill Patterson—as the newspaper evolved from its publication by the entrepreneurial printer-editor to the great corporations that today are attempting to meet the stiff competition of radio and particularly television. Rutland effectively, though unobtrusively, melds in the changes in newspaper technology and the working conditions of newspapermen. His clearly written, swift-flowing narrative sparkles with a sprightly style enhanced by straightforward, sometimes even blunt, language. He is especially good in weaving the main historical events of the time into his account, revealing how often the newspaper has been a clear reflection of American life

and thought. By using descriptive details and striking quotes from editorials and news columns, along with new facts and interpretations, the author underscores the significance, or heightens the drama, of men and events. Along the way he suggests areas worthy of further study. His research is based on excellent secondary sources, numerous newspapers, and many of the more recent scholarly studies. In a provocative final chapter Rutland raises, though he does not answer, many of the right questions.

Contrary to Rutland's assertion that his work is not a conventional history, it is just that, since most of the basic problems and issues newspapers and newspapermen have faced are left virtually untouched, for example, the influence of the advertiser and the newspaper business office, as well as that of the publisher himself, on political, social, and other views expressed by the press. Slanted news Rutland would have us believe only emerged with the appearance of *Time* and *Newsweek* magazines in the 1920s and early 1930s, and that only in 1933 did the debate on newsroom philosophy really begin.

By taking up weeklies such as *Time* and *Life* and monthlies like *McClure's* and *Reader's Digest*, Rutland can be accused of not having included all such types. Other aspects left out or hardly mentioned include the development of the foreign correspondent, the decline of the editorial and religious and ethnic newspapers, and the development and significance of the cartoon and the letters-to-the-editor feature. Finally, his research shows little use of trade journals, such as the *Journalist*, the *Fourth Estate*, and *Editorial Publisher*, and he utilizes only a few histories of individual newspapers and works by newspapermen themselves.

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BARBARA GUTMANN ROSENKRANTZ. *Public Health and the State: Changing Views in Massachusetts, 1842-1936*. Cambridge, Mass.: Harvard University Press. 1972. Pp. 259. \$9.00.

J. DOUGLAS BROWN. *An American Philosophy of Social Security: Evolution and Issues*. Princeton: Princeton University Press. 1972. Pp. x, 244. \$8.50.

Several excellent monographs have been published in recent years dealing with the history of public health. These include John Blake's *Public Health in the Town of Boston, 1630-1822*; Charles Rosenberg's *The Cholera Years*; James H. Cassedy's *Charles V. Chapin and the Public Health Movement*; and John Duffy's *A History of Public Health in New York City*. Apart from the focus on Massachusetts and the evolution of its Board of Health, the Rosenkrantz volume does not add much to the fund of knowledge. There are some useful biographical accounts of health reformers and administrators: Henry Shattuck, Edward Jarvis, Henry Bowditch, George Derby, and Henry Pickering Walcott (chairman of the Massachusetts State Board of Health, 1886-1914, and "prototype of the physician-statesman"). But the book suffers from the absence of a significant conceptual framework.

I gather the author was trying to say that nineteenth-century public health reform was permeated by humanitarian and moralistic zeal, as compared to twentieth century "substitution of scientific for ethical objectives" and "explicit denial of responsibility for social reform." This line of analysis, while perhaps valid, is also platitudinous. As social or intellectual history, the book's value is limited to variations on a familiar theme.

Public health problems forced Americans in the nineteenth century to expand the welfare and service functions of government. It was not until the 1930s, however, that a relatively coherent, national system of economic security was established. *An American Philosophy of Social Security* is by an economist, a participant in the drafting of the Social Security Act in 1935 who has remained active in the formulation of policy and legislation. Brown deals primarily with the contributory programs, exclusive of unemployment insurance. These include old-age, survivors, and dependents benefits, disability insurance, and Medicare. Although he is concerned mainly with contemporary policy issues, his study has utility for the social welfare historian. Brown frequently refers to constitutional and political factors of the 1930s that influenced the content of the Social Security Act—a dramatic product of American social politics which bal-

anced expediency with long-range objectives.

While useful to historians, the book is indispensable to those concerned with contemporary welfare policy. Brown explores many fundamental issues: the relationship between the social insurances and public assistance; risks appropriate to the social insurance mechanism; the financial role of the federal government; criteria for determining individual and family benefits; the expansion of Medicare into a broader health insurance system. He recommends many improvements in the benefit structure but always in the context of political and economic feasibility.

Brown's view of the social security system as an organic historical phenomenon is akin to a legal scholar's view of the constitution. It must respond to changing circumstances but its integrity must be protected. This means that its functions should not be confused with those of public assistance and that it should not be burdened with costs or responsibilities inappropriate to a wage-related insurance program. It should not be used, for example, to compensate for low wages or confused with the private retirement annuity. The system is designed not to replace lost earnings in their entirety but to replace "that degree of loss which is socially undesirable." This policy justifies weighting benefits in favor of lower-income contributors, but it does not justify using the social security system as a substitute for effective national employment policies.

It is unfortunate that politicians cannot be required to read this volume; piling payroll costs and benefits in a ritualistic fashion onto the social security system has become fashionable as a means of buying votes every two years. The danger is that political expediency might blur the distinction between social insurance and public assistance or might otherwise compromise the distinctive equity, the contributory, contractual, and wage-related character of the social insurances. "Because social insurance is effective in the limited burden for which it is intended," Brown cautions, "does not justify placing burdens upon it which distort its purpose and endanger its acceptance."

ROY LUBOVE

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KENNETH J. BERTRAND. *Americans in Antarctica, 1775-1948*. (American Geographical Society, Special Publication number 39. Published under the support of the National Science Foundation.) New York: the Society. 1971. Pp. xvi, 554. \$25.00.

In an age like ours of hurriedly written publications it is a pleasure to encounter a work of thorough scholarship. Professor Bertrand, a geographer, has had a long association with his subject, beginning in 1946 with his assignment to the Special Committee on Antarctic Names under the Department of Interior and an earlier publication, *Geographic Names of Antarctica* (1956). Now, in the work under review, he offers a new departure point for readers interested in American Antarctic exploration. Previously the choices were encyclopedic digests, selected chapters on various expeditions, collected biographical sketches of certain explorers, or the published monographs of particular explorers. Serious readers may forthwith begin with Bertrand's work and proceed to the desired depth of study under his guidance.

Using the Antarctic Convergence as the physical boundary for his study and the activities of the eighteenth-century American sealers as the chronological beginning, Bertrand carries his scholarly summary of both American exploration and scientific investigation through the exploration year 1947-48. The resulting twenty-four chapters are well proportioned, and their varying lengths provide a rough quantitative measurement of the significance of each explorer or expedition.

Controversy is a characteristic in the history of Antarctic exploration and scientific investigation. Who first discovered Heard Island? Did the United States Exploring Expedition under Charles Wilkes actually see the "eastern land-falls" and, hence, prove the existence of the Antarctic continent? Is Antarctica one or two land masses? Why are the Bunger Hills ice free? Bertrand treats judiciously these representative controversial questions.

Especially useful are both the notes, sometimes extensive, and the bibliographies appearing at the end of each chapter. While the bibliographies may not be definitive they come close to that unattainable ideal. Of particular value are the range and depth of manuscript

material comprising letters, journals, logbooks, ship registrations, crew lists, custom house records, and government reports.

Lastly, two deficiencies are manifest. Ironically the twenty-six maps supplementing the text are insufficient. With the multitude of place names cited, there is the need for one or two large, detailed folding maps in keeping with the cartographic standards of older American Geographical Society publications. And given Professor Bertrand's long association with his subject one may only wish that he had provided more interpretation. While each chapter commences with the significance of its subject the result is fragmentation, and one misses the larger interpretation that the erudition, reflected within the book, might have provided.

WALTER L. BERG

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EDGAR J. MCMANUS. *Black Bondage in the North*. [Syracuse:] Syracuse University Press. 1973. Pp. xiii, 236. \$9.95.

Professor McManus's volume examines slavery in New England and in the Delaware and Hudson valleys—its origins, its place in the Northern colonial economy, its legal underpinnings, how it operated as a system of servitude, the ways in which slaves reacted to it, and its ultimate decline in the late eighteenth century. Those familiar with Lorenzo J. Greene's *The Negro in Colonial New England* (1942), McManus's own book on slavery in New York, the more recondite studies of slavery in Pennsylvania and New Jersey, Benjamin Quarles's *The Negro in the American Revolution* (1967), and Arthur Zilversmit's *The First Emancipation* (1971), will find little new information. Yet the author presents us with a useful and thoughtful synthesis.

On the whole this volume is factual rather than analytical or interpretive. In fact the preface informs us that "an attempt has been made to tell the story with a minimum of generalization and interpretation. . . . Since this is a pioneer [*sic*] study, it seemed more important to define factual boundaries than to plunge into theoretical thickets." Yet in a concluding chapter McManus plunges into the thicket of comparative slavery in the New World. Adopting a rather simplistic economic

interpretation he concludes that differences in the character of slavery were based on the type of work that slaves performed. Black artisans with specialized skills, such as were unusually common in the colonies analyzed in this book, possessed "leverage" that secured them "a privileged position within the system." Similarly, though in my judgment his evidence would not fully support his conclusion, McManus dismisses the ideological factors emphasized by Zilversmit in explaining the abolition of slavery in the North. Instead McManus attributes the demise of the institution there simply to a decline both in the economic importance of slavery and in the proportion of blacks in the population.

Although one must thus enter reservations concerning the analytical sophistication of this volume, and although the author has unfortunately not included a discussion of slavery in the Old Northwest, he does offer a long-needed factual synthesis of the subject, based upon a wide array of monographic and primary sources.

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JAMES ROBERT ENTERLINE. *Viking America: The Norse Crossings and Their Legacy*. Garden City, N.Y.: Doubleday and Company. 1972. Pp. xix, 217. \$6.95.

FREDERICK J. POHL. *The Viking Settlements of North America*. New York: Clarkson N. Potter; distrib. by Crown Publishers, New York. 1972. Pp. xii, 339. \$7.95.

Our knowledge of Scandinavian contacts with the North American continent before Columbus is based upon two different kinds of information. One consists of scattered references found in Icelandic and medieval European sources and certain late medieval and early modern maps, of which the two well-known *Vinland Sagas* are the most important. The second is composed of archeological evidence from Greenland, Northern Newfoundland, the Ungava area of Labrador and certain Canadian Arctic Islands, some of which is relatively recent and still difficult to interpret. There have also been a few isolated archeological finds of Norse material beyond the above northern American area of which only that uncovered in the James Bay region of Northern Ontario has been accepted without question. Furthermore the major written sources for our knowledge of such

contacts, the *Vinland Sagas*, suffer from the fact that they relate events that took place two centuries before they were written down or composed by men who had no direct knowledge of the geographic areas they were describing.

Both of these books by Enterline and Pohl attempt to use this scattered and often unsatisfactory material to present us with a coherent picture and both share an apparent belief that the *Vinland Sagas* present an accurate picture of parts of the coastline of the North American continent, which were visited and briefly settled by Scandinavians around the year 1000 A.D.—surely a rather dangerous assumption. If one accepts, however, the general accuracy of this saga evidence, and there is some reason why we should be willing to do so in the light of archeological discoveries, much of Enterline's book seems relatively satisfactory and his initial thesis quite probable.

This thesis is that the *Vinland Sagas* show that the Helluland, Markland, and Vinland they mention were located along Baffin Island and the Ungava Bay area of northern Labrador. Especially probable is his contention that somewhat later on in the twelfth, thirteenth, and fourteenth centuries the Scandinavian Greenlanders of the northern settlement who included many hunters advanced north along the Greenland coast and proceeded into the central Arctic by way of the strait between Devon Island and northern Baffinland towards the Coppermine River area of Canada. There is archeological evidence of their presence along this route.

His attempt, however, to link the disappearance of the inhabitants of this northern settlement later on with their absorption into an Eskimo population advancing east from Alaska seems more doubtful. So, too, is his thesis that cartographic features depicted on the Vinland and other late medieval and early modern maps were derived from knowledge initially provided by these same Eskimos. We would do well also to question seriously his contention that there was a knowledge of the Labrador coastal region in Western Europe before Columbus, which was derived from contacts with the dying fourteenth- and fifteenth-century Greenland colony—though this is possible.

On the other hand, Pohl's attempt to link

evidence from the *Vinland Sagas* to a series of specific locations along the eastern Atlantic seaboard of North America seems much more unsatisfactory, especially since none of the archeological evidence he provides can be termed conclusive, nor can one accept his reading of the Vinland map as definitive. Perhaps all that can fairly be said of his thesis, as advanced in this and other books, is that it may turn out to be true but must at this time be termed extremely doubtful until more concrete evidence is provided for us.

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LARZER ZIFF. *Puritanism in America: New Culture in a New World*. New York: Viking Press. 1973. Pp. xiv, 338. \$10.00.

In yet another attempt to understand the Puritans, Larzer Ziff has traced, perhaps more effectively than any previous writer, the subtle matter of interrelationship between word and act, minister and people, New England theology and New England behavior. Viewing Puritanism as a particular body of ideas held by dominant members of society, influencing and reflecting the way men lived their common lives and reacted to their daily problems, he defines this combination of ideology and social pattern as Puritan culture, a concept that provides the theme for the book. Not a factual history, it is, rather, a discursive, often impressionistic, interpretation.

Puritans, as Ziff sees them, were liberated from both corrupted church and government by their sense of being a chosen people possessing the potential of saving grace and preserving the pure word of God. Such a belief met the needs of their condition, providing them with a new culture in an age when thousands found themselves masterless and landless in the face of economic forces and political orders that did not satisfy. In the New World where they hoped to construct a better social, economic, and political order they shaped an ideology that was appropriate to their conditions of life.

How the Puritan applied his ideology is the story of Puritan culture in New England. The author ranges with searching intelligence and a profound knowledge of New England's his-

tory to show how Puritans reacted to domestic problems, like the antinomians, Roger Williams, the Indian wars, or the witchcraft outbreak; how they thought on abstract matters like love and death; and what their response was to developments abroad like the Civil War, the recall of their charter, and the wars of empire.

Changes that came in the colony are linked with changes in the thinking of the leaders. Offering a number of challenging and revealing interpretations on subjects like the witchcraft frenzy and the Great Awakening in New England, the author also hazards a few conjectures about the persistence of Puritan culture in later American life and letters.

The author's method may be adequate for literary scholars, but historians will want better evidence than inferences about ordinary life in the colony drawn from the writings of leaders. Rich and metaphorical in language, Ziff's literary figures will not satisfy those who seek precision in meaning, not to mention quantitative data. He acknowledges reliance on the work of many scholars, but mentions only Palfrey, the Adamses, and Miller. In this largely interpretive study one looks in vain for the insights of the behavioralists. The author's interpretation rests often on appearances, the mere correspondence of ideas and actions. Readers without an extensive background in the subject will find this book hard going, although its grace and subtlety do, in the end, appeal.

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SEYMOUR VAN DYKEN. *Samuel Willard, 1640-1707: Preacher of Orthodoxy in an Era of Change*. Grand Rapids, Mich.: William B. Eerdmans Publishing Company. 1972. Pp. 224. \$5.95.

Samuel Willard was clearly an important character in Puritan New England. From his position as pastor of Boston's South Church, and, later, as acting president of Harvard College, Willard spoke out on the major religious-political issues of his day. He became personally involved in many of the important events he witnessed. Seymour Van Dyken pictures Willard's public life—his sermons, writings, participation in events—as an effort to defend the

orthodoxy of the founding generation in a way that would adapt it to each contemporary crisis without compromising its fundamental principles.

It is this approach to understanding Willard's life that dominates Van Dyken's book. His view of Willard as a preacher of orthodoxy in an era of change, it seems to me, is much like that of denominational historians who portray the reactions of their small denomination to events in the world around them as the history of their group. For much of his book (certainly the first half) Van Dyken's goal is to explain how Willard fit into and effected his "era of change." The author's approach tends ultimately to distort the context in which historical events occurred and to exaggerate the separation between Willard and the larger community in which he lived. The result is a rather separated and repetitious account of seventeenth-century Puritan history. Unable to integrate his subject and his subject matter, Van Dyken loses the chance to use his biography to raise meaningful questions, even if only indirectly.

The second half of the book is more satisfying. The final five chapters are a straightforward discussion of religious doctrine. As a history of ideas, it is very well done. Van Dyken offers us in detail, and with skill, an account of the theological principles of late seventeenth-century orthodoxy as represented by Samuel Willard. The latter part of the book is of considerable value to historians.

DAVID KOBRIN

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A History of Missouri. Volume 1, 1673 to 1820, by WILLIAM E. FOLEY; volume 2, 1820 to 1860, by PERRY MCCANDLESS. (The Missouri Sesquicentennial Edition.) [Columbia:] University of Missouri Press. 1971; 1972. Pp. ix, 237; ix, 325. \$9.50 each.

The above volumes are the first two of the five-volume Missouri Sesquicentennial History, a joint project of the University of Missouri and the University of Missouri Press; the remaining three volumes are projected for publication by 1976, the 150th anniversary of Missouri's statehood. The authors of volumes 1 and 2, William E. Foley and Perry McCandless, are both professors of history at Central Missouri

State College at Warrensburg. William E. Parish of Westminster College at Fulton is the general editor of the series, the purpose of which is the presentation of a broad survey of the political, social, economic, and intellectual trends that have dominated the state's history. With only five hundred pages of printed text allotted to the two authors they have done well in their attempt to allocate the space and still present a balanced account of all phases of the state's early history. Though concise and straightforward with little detail, the presentation is still interesting and readable. One wishes there had been space for more detail, but on the whole I feel the authors have fulfilled their assignment well.

The absence of footnotes, except for a few of an explanatory nature, will prove a disappointment to the serious student but will no doubt meet the approval of the general reader. Sources used in the preparation of the volumes are listed in the bibliographies at the end of each volume; these are also intended as a guide for additional reading.

Professor Foley has chosen to devote only three of his ten chapters to the period before 1803 when Missouri (or "Upper Louisiana" as it was first called) was successively under French and Spanish control and instead has placed the main emphasis on the last seventeen years of the somewhat turbulent territorial period. This treatment is in contrast with the pre-statehood histories of the states east of the Mississippi in which much space has been given to the French and British occupation. The principal result of the Spanish occupation was the land grants that were made; the claims that evolved out of these grants continued to plague American officials for many years before they were finally settled.

The interesting constitutional questions that arose over the purchase of the Louisiana Territory by the United States and the setting up of a new government for the region form one of the best chapters in volume 1. The significance of Missouri as the gateway to the West was foretold by the presence of Meriwether Lewis and William Clark in St. Louis in March 1804. They were making preparations for their expedition up the Missouri at the same time that the American flag was being raised in the city, signifying the transfer of the area to the

United States. These and other explorers would in time be followed by those seeking fame and fortune in the West, and finally by the Conestoga wagons filled with emigrating families. The unique geographic position of the state, strategically located between the East and West, was to prove helpful to its later development. Similarly, however, its central position between the North and South caused Missouri to become deeply embroiled in the slavery issue.

Professor McCandless begins his volume with Missouri's struggle for statehood, the approval of which was delayed for almost two years by the debate in Congress over the slavery issue, finally resolved by the Missouri Compromise. Again in the late 1840s and in the 1850s, during the debates over slavery in the territory gained from Mexico and the admission of Kansas and Nebraska, Missouri was drawn into the maelstrom of a national issue that had an effect on local problems.

In between his consideration of the slavery issue Professor McCandless had the opportunity to discuss other political issues and their exponents, including the emergence of Jacksonian Democracy in the 1830s and its continued supremacy down to the time of the Civil War. Other chapters deal with the people who made up the state (most of whom came from the South), their occupations, the cities they built, their search for better educational opportunities, the emergence of a feeling of responsibility for those less fortunate, and their ability to express themselves in literature and in art.

The typography used in the volumes is pleasing to the eye and adds much to their readability. The indexes seem inadequate; for example, in volume 2 there are sixty page references to Thomas Hart Benton without any subheads. The volumes would have been enhanced by the addition of illustrations and maps; the one or two maps that appear are too small to be of help.

DOROTHY RIKER

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History of the Supreme Court of the United States. Volume 1, Antecedents and Beginnings to 1801. By JULIUS GOEBEL, JR. (The Oliver Wendell Holmes Devise.) New York: Macmillan Company. 1971. Pp. xxv, 864. \$30.00.

At long last the Holmes Devise series of the Supreme Court is launched, and rather uniquely, with Julius Goebel's volume on antecedents and the pre-Marshall Court. A product of more than a decade of intensive research and a lifetime of general scholarship, Goebel's *magnum opus* will titillate the specialist, but frustrate the generalist. Crammed with little or unknown facts about the origins and procedures of the judiciary in America, as well as rich in unadulterated legal history, the book is surprisingly thin on the substance of the Supreme Court itself.

In many ways Goebel's book is legal and constitutional history in its best and purest form. He provides rich detail on the reception of common law in America, the traditions of colonial judicial control over legislation, the evolution of judicial review, the position of the judiciary in colonial and revolutionary America, the debates about the judiciary in the drafting and ratifying conventions, the conflict over the Judiciary Act of 1789, the ingredients of the process acts, and the procedural business of the circuit courts. The freshest part of Goebel's study, and the most exasperating, concerns practice and procedure before early American courts—colonial, state, and federal. Even the heretofore obscure process acts that accompanied the Federal Judiciary Act of 1789 are examined with remarkable thoroughness. From this patient viewing Goebel concludes that the federal statutes were significantly influenced by a variety of state procedures. For example, Congress, influenced by state precedent, departed from the English rule that service had to be made in person and instead provided the rule that service could be perfected by leaving a copy of the process at the defendant's abode. But unlike state provisions, section 7 of the Senate committee's initial process bill made jury trial available to either party "regardless of the nature of the action, and [the] defendant was specifically given the opportunity to set aside the default judgment simply by an appearance within the instant term and payment of costs" (p. 530). One suspects an eighteenth-century lawyer, faced with innumerable technical hurdles and ceremonial pitfalls would have emerged from federal court battle unscathed after a single reading of Goebel.

Other sections, in spite of their awesome particularity, lack contextual substance. Goebel's discussion of the circuit courts provides much information about the revolt against circuit riding, original civil jurisdiction, judicial review of state and federal statutes, removal of cases from state to federal courts, and criminal and admiralty jurisdiction, but little about the tribunals in the constitutional reality and politics of the times (save for a brief section on "The Attacks on the Judicial"). How extensive was the business of the courts? How did the litigating public respond to the circuit courts? Did the courts favor absentee landlords and foreign creditors? What was the place of the courts in the Federalist-Republican conflict?

A more serious deficiency in a book constituting the first volume of a multivolumed history of the Supreme Court is the rather shallow treatment of the business and significance of the high tribunal itself. Only 71 of 793 pages are devoted to what the author terms the Court's "political and constitutional issues," with another 59 pages concerned with rather technical questions of jurisdiction and procedure. Illustrative of the book's shortcomings in this area is Goebel's treatment of perhaps the most important case of the Court's first decade, *Chisholm v. Georgia*. The author spends too much space on summaries of counsel's arguments and opinions of the justices and not enough on the background and settlement of the conflict. Goebel would have profited from a reading of Doyle Mathis's excellent studies of the case published in 1967 and 1968.

Surprising also is Goebel's failure to utilize expeditiously the Jay Papers housed at his home school, Columbia University, and the short but revealing study of Jay by his colleague, Richard B. Morris. Because of this inattention the book is void of material on Jay's accomplishments as a presidential adviser and judicial administrator as well as on the impact of his political and judicial philosophy. Greater use of the papers, too, might have furnished more information on the internal proceedings of the Court, including the give and take of judicial politicking on and off the bench.

It perhaps follows that with Goebel's preoccupation with roots and procedure he fails

to define adequately the significance of the Court as an institution in the first decade of the new nation. Was the high tribunal simply a preliminary footnote to the Marshall era, as earlier historians have suggested, or important in itself, as more recent scholars argue?

Those historians not trained in the law will find this study hard going. And those scholars interested in determining the place of the Court in the constitution and politics of the first decade of the new nation will be disappointed.

ROBERT M. IRELAND

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THOMAS H. POPE. *The History of Newberry County, South Carolina*. Volume 1: 1749-1860. Columbia: University of South Carolina Press. 1973. Pp. xvii, 389. \$17.95.

Local history ought to be history at the grass-roots, detailing the social, economic, and political development of a single community and providing a basis for generalizations about regional and national trends. Thomas H. Pope has written an outstanding example of local history that transcends parochialism and family pride. His study of Newberry County in its transition from frontier to plantation will be of real value to historians. It is grass-roots history, written in the context of regional history and based on thorough research.

Newberry County was created in 1785 from an area in the fork of the Broad and Saluda rivers. It was settled by Scotch-Irish and German pioneers, coming from Pennsylvania, Maryland, and the Shenandoah Valley of Virginia. German-speaking Brethren (Dunkers) from Maryland settled in the Dutch Fork and Quakers along Bush River. The first Baptist church was organized from a Rockingham County, Virginia, congregation.

The author describes the back-country society of the eighteenth century and the county's share in the Cherokee War and the regulator movement. The county was the scene of partisan warfare in the Revolution, with 500 men from the Dutch Fork enlisting in the South Carolina Royalist Regiment.

From a region producing flour, beeswax, furs, tobacco, cattle, and hides for market, Newberry County shifted to a cotton economy in the nine-

teenth century. Between 1800 and 1860 the white population dropped from eleven to seven thousand and the slave population rose from two to fourteen thousand. The city of Newberry became the leading inland cotton market of South Carolina, as well as early rail center.

Although written during the leisure moments in a busy career of an attorney and state legislator, Pope's book can serve professional historians as a model in the imaginative use of local records. The author has also used monographs to good advantage. Unfortunately the recent study of the Church of the Brethren in the Carolinas by Roger Sappington of Bridgewater College appeared too late for inclusion in the book, and Pope's treatment of the Dunker settlement is thin by comparison.

The book is written in an attractive style and enriched by handsome photographs of county landmarks, mainly dating from the antebellum period, but including a few earlier buildings as well.

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WILLIAM B. WILLCOX *et al.*, editors. *The Papers of Benjamin Franklin*. Volume 16, *January 1 through December 31, 1769*; volume 17, *January 1 through December 31, 1770*. (Sponsored by the American Philosophical Society and Yale University.) New Haven: Yale University Press. 1972; 1973. Pp. xxiv, 359; xxxii, 430. \$17.50 each.

As the massive project of assembling the Franklin Papers approaches its halfway mark, the character, the mind, and the temperament of the man shine through every volume; and there also shine through every volume the superior scholarship of the editors, their lucid insight, and their literary skill.

The editors continue to resist any temptation to use the edition to construct an exegesis of Franklin's writings, to impose upon them extraneous interpretations, or otherwise to encumber them with obiter dicta. These volumes, like their predecessors, evince the editors' constant awareness that their function is wholly different from that of the historian. To have value, history must distill meaning from the experience of a people or an era. Its distinction consists not only in the quality of its scholar-

ship but also in its validity as what is essentially a work of art, in that—like all works of art—it is the product of a gifted or knowing way of looking at things, of giving point and emphasis to reality. The task of the editor of documents and papers is quite different: to gather and present in useful form the raw materials of history. He must not intrude upon the substance of those materials. At the same time he must place them in time and in context, identify often obscure individuals, events, and loci, and furnish frequently elusive bibliographical data—all this in fidelity to a consistent editorial design and a reliable methodology, while scrupulously allowing the papers to speak for themselves. Certainly no editors have taken a more disciplined view of their high function and carried it out with more professional skill than the editors of the Franklin Papers.

The present volumes embrace two events of the utmost significance in the life of Franklin, in the imperial scheme of the British establishment with which he was negotiating, and in the vexations of the American colonies, four of which he was representing by 1770. The first of these was the collapse of the brief, hapless ministry of the duke of Grafton, followed by the long, catastrophic ministry of Lord North, after whose rise to power Franklin's views underwent a significant change: he denied explicitly, as he had not done before, the claim of Parliament to legislative authority over the colonies, his position being that, like all Britons, Americans were subject to the Crown but not to a Parliament in which they had no representation. His idea of a British Empire became an eighteenth-century vision of the twentieth-century Commonwealth. And by 1770 he placed all his hopes in the Crown. "Let us therefore hold fast [our] Loyalty to our King (who has the best Disposition toward us, and has a Family-Interest in our Prosperity), as that steady Loyalty is the most probable Means of securing us from the Arbitrary Power of a corrupt Parliament. . . ."

The second shattering event of 1770 was the first ominous action of the determinedly inept North ministry: the repeal of all the Townshend duties except that on tea, the exception being made not as a revenue-raising measure but as a gratuitous reminder of Parlia-

ment's right to impose taxes on the colonies. Repercussions to this blunder were not to be felt fully for some time, although it had an immediately deteriorative effect on Franklin's strategy of maintaining colonial nonimportation agreements to advance the economic independence of the colonies, thus depriving him of his most prized weapon to combat British political arrogance.

Both his public papers and private correspondence in these volumes reveal anew how thoroughly and how longingly Franklin was an Englishman—not by heritage alone (his father was twenty-six when he left Northamptonshire for Boston), but also by temperament and by as deep feelings as his bemused and detached nature permitted. Although he had moments of acute political uneasiness in Whitehall, he was wholly at home in England, fond of Grafton and Lord Chatham, and appreciative of the honors the English learned community had heaped upon him; and, as late as 1768, he had looked with favor upon his rumored appointment to the service of the Crown as undersecretary of state for America. Yet events and his own resiliency were to make him also thoroughly, inherently, and devotedly American—even to the estrangement of his only surviving son, who remained to the end loyal to the Crown. A Daedalian man and a totally absorbing one, Franklin has a place in history that is well served by this masterly edition of his papers.

ARTHUR BERNON TOURTELLOT
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JACKSON TURNER MAIN. *The Sovereign States, 1775-1783*. New York: New Viewpoints. 1973. Pp. vii, 502. Cloth \$10.00, paper \$4.50.

The bicentennial of the American Revolution is two years away, but the floodgates of historical writing already have been thrown open and the deluge is beginning to roar down upon us. Main's book is part of the flood, the bulk of which seems intended for that elusive creature known as the "general reader." This book is also aimed at the nonspecialist. It is therefore devoid of footnotes, and the index is skimpy. The time frame is the war period, and the topics covered are of a standard order—for example, the economic, political, and social im-

pact of the war; the formation of constitutions and new governments; and the loyalist problem. There is one striking (and welcome) omission—nary a word on military history. The basic organization, exclusive of the introductory chapters, adheres to the familiar macrocosm-microcosm format. Main begins with broad strokes and then moves to a state-by-state summary of what transpired. Complementing the text is a lengthy bibliographical essay, arranged under chapter headings. This section contains many recent significant works and, because of Main's incisive annotations, holds as much value for the specialist as it does for the general reader.

Because the book is part of a series that falls under the rubric of "new viewpoints," it is incumbent upon a reviewer to address himself to this point. Main's principal "new viewpoints" are not exactly novel, but they are away from the "mainstream" of current historiography of the Revolutionary era. Thus he places an added value on the importance of the British governors and executive departments and correspondingly downplays the power and significance of colonial assemblies, a favorite theme of modern scholars—although, in a later section, he backtracks a bit, thereby weakening his position. On the well-known "conflict" or "consensus" issue (that is, should one emphasize that which divided or united the rebels. Main steers a middle course between these two divergent interpretations and acknowledges the validity of both, with some minor modifications. If his position is not novel, it at least has the virtue of conforming to common sense. He offers some penetrating and arresting theories on the economic and political development of the embryonic states, and these sections constitute creative scholarship of a high order. He also takes exception to the Franklin Jameson school of interpretation on the social consequences of the Revolution. The factor of incessant social mobility, Main asserts, mitigated the effects of the demise of the loyalist leaders and other social upheavals.

Viewed as a study for the average American, this book is a solid success; and specialists will profit from Main's chapters on economic and political developments. A consummate scholar who has researched this period in depth and utilized many primary resources, Main has pro-

duced an authoritative account that supersedes the older standard work of the period, Allan Nevins's *The American States During and After the American Revolution, 1775-1789* (1924). Main has set a high standard for the rash of surveys destined to appear before Washington once again says farewell to his officers in 1983 and the bicentennial comes to an end.

LOUIS LEONARD TUCKER
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HOWARD C. RICE, JR., and ANNE S. K. BROWN, translated and edited by. *The American Campaigns of Rochambeau's Army, 1780, 1781, 1782, 1783*. Volume 1, *The Journals of Clermont-Crèvecoeur, Verger, and Berthier*; volume 2, *The Itineraries; Maps and Views*. Princeton: Princeton University Press; Providence: Brown University Press. 1972. Pp. xxviii, 351; v, 204, 177 plates, 345-62. \$100.00 the set.

CATESBY WILLIS STEWART. *The Life of Brigadier General William Woodford of the American Revolution*. In two volumes. Richmond, Va.: Whittet and Shepperson. 1973. Pp. xiii, 743; 745-1446. \$30.00 the set.

Both of these large, handsomely bound, two-volume works deal with the American Revolution, and both are chiefly valuable for their documents, not their narrative or interpretation. There the similarity ends. The Rice and Brown work is meticulously edited and carefully and logically arranged, whereas the Stewart biography of William Woodford seems more like a series of notes put together with scissors and paste.

The first of the volumes in *The American Campaigns of Rochambeau's Army* consists of the journals of three French junior officers who came to Newport with Rochambeau in 1780 and participated in the Yorktown campaign and later in the operations in the Caribbean. The second volume contains the itineraries prepared by the engineers of Rochambeau's army on the march from Newport to New York to Yorktown and maps and views that include routes and encampments on both the march to Yorktown and the return trip to New York and Connecticut.

The three journalists are the Comte de Clermont-Crèvecoeur, a first lieutenant in the Auxonne Regiment; Jean-Baptiste Antoine de Verger, a sublieutenant in the Royal Deux-

Ponts regiment of infantry; and Louis Alexandre Berthier, a captain attached to the Soissonnais regiment as assistant quartermaster general. Some portions of these journals have been published before, but this is the first complete edition, in either French or English. Indeed it was only by some clever detective work that the author of the first journal was identified by Rice and Brown as Clermont-Crèvecoeur; parts of the journal had previously been published as the work of one De Robernier, a subaltern in the Soissonnais regiment. In addition to the three journals printed here, an appendix to the first volume contains a most helpful checklist of all other extant writings of French soldiers who served in the American Revolution, either with Rochambeau's army or with Washington's.

Of the three journalists Berthier was the only one to achieve any great measure of fame in the world as one of Napoleon's leading generals. Clermont-Crèvecoeur and Verger, though both pursued military careers on their return to Europe, were numbered among the foes of the French Revolution and never escaped the relative obscurity of emigré colonels and generals. Yet it is the journal of Clermont-Crèvecoeur that holds the greatest interest for the general reader with its comments on American flora and fauna and on the strange customs of the natives. One is surprised, for instance, to learn from him that French officers after the surrender at Yorktown got along "famously" with their British counterparts but seldom associated with the Americans whose customs were so different from their own, or that Virginia women of twenty would pass for thirty-five in France. Berthier, on the other hand, writes more prosaically, if quite accurately and precisely, of military matters, as does Verger, though the latter repeats all sorts of outrageous stories about British atrocities and accepts many myths about America and Americans at face value. All three note, with almost similar language, that peculiar American animal, the opossum, that carried its young in a pouch.

All in all this work provides the student of the American Revolution with an admirable source book on the French army in America. The arrangement is excellent, the translation well rendered, the explanatory notes informative, and the reproduction of the French maps

extraordinarily good as one follows Rochambeau's army through some fifty encampments. The introductory biographies give brief but adequate accounts of the men who wrote the journals. There is little a reviewer can quarrel with in these two volumes except the price, which will make it difficult for even libraries to acquire the set.

Mrs. Stewart's biography of William Woodford, one of the Virginia brigadiers of the Revolution, is not nearly so successful. She has, it is true, in documents and text, illumined the life of Woodford and his contemporaries, social and political life in eighteenth-century Virginia, and military affairs during the Revolution; and for this reason the two-volume biography will be of some interest to historians. But the whole work is something less than adequate as historical narrative, consisting of a formidable number of printed documents or excerpts from documents with inadequate connective tissue between them. The work lacks logical organization and the style is rambling. Genealogists, antiquarians, and historians will find here, however, a compendium of information if they dig hard enough.

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JAMES McLACHLAN. *American Boarding Schools: A Historical Study*. New York: Charles Scribner's Sons. 1970. Pp. x, 381. \$10.00.

STANLEY K. SCHULTZ. *The Culture Factory: Boston Public Schools, 1789-1860*. (The Urban Life in America Series.) New York: Oxford University Press. 1973. Pp. xvi, 394. \$11.50.

The contemporary financial plight of many U. S. elite secondary schools and colleges suggests a need for thorough inquiry into their reason for being. This coupled with the conventionally strong urge to admiration for the public school in its classic relationship to the American Dream emphasizes the desirability of a prompt review of each sector. James McLachlan's *American Boarding Schools* fills rather adequately this first requirement and Stanley K. Schultz's *The Culture Factory*—despite its clumsy title—the second. Both authors are to be complimented for their aspiration to place profiles of the national record within the main-

stream of American history—a performance most frequently bypassed by the upper echelons of the country's historiographers to the considerable eclipse of American educational achievement.

Following sociologist Digby Baltzell's lead, McLachlan limits his field to fourteen down-East college preparatory schools (thus omitting two Virginia institutes not especially pertinent to the study). These include Phillips Andover, Phillips Exeter, Hill, St. Paul's, St. Mark's, Lawrenceville, Groton, Taft, Hotchkiss, Choate, St. George's, Middlesex, Deerfield, and Kent. The book is arranged around three sections: Federalists, Victorians, and Progressives, with 20 pages of engravings and photographs, and 65 pages related to notes and sources.

Beginning with a brief summary of the intellectual climate, he attempts to relate—through vignettes of leading prep school headmasters—educational philosophy in the American East with the newer pedagogical thought then stirring in Western Europe of the early 1800s. Pursuing this ideological pattern into a retrenchment (Victorian) period, McLachlan judges the emergent philosophy in the schools he has selected as “progressive,” a conclusion justified only by referring pedagogical developments to the political climate—hardly an expression of thought about school practice, which remained (and this its saving grace) largely classical and idealistic.

He shows curriculum models, generally replicated by these private Etons and Harrows of America (although strongly disputing such association), and reiterates headmasterly ideology bent on setting the day's work toward the goal of “preparation for life”—here interpreted as a classical mode paralleling the old Fürstenschulen of Prussia, yet with a strong emphasis on sports and other group activities. Quoting Domhoff it appears that the prime value of the select school is the inculcation of “upper-class value, upper-class manners, and most of all upper-class speech.”

Nonetheless McLachlan's focus remains myopic; with other than this select group of chiefly New England boarding schools the target, further academy treatment, especially that of female education, is left in obscurity. Little of the color of boarding school's daily life is conveyed, and indeed one might gain more of this

from a quick reading of Mr. Chips or John Knowles's *Devon*. Neither does McLachlan bring his account to the current innovation in coeducation (recently at Exeter and St. Paul's) nor greatly stress the recent urge to admit more black students and members of other minorities—Exeter, for example, now expending half a million annually in diversified scholarships. Sizer's *Age of the Academies* appears to have been little used or the popular historians of American education—Butts, Good, Meyer, Mulhern, or Commager—consulted.

Schultz's work, subtitled *Boston Public Schools, 1789-1860*, presents in summary the development or maturation of this city's elementary school system from its colonial legacy to the inauguration of the urban superintendency and the enactment of the compulsory attendance laws, first in the U. S. Five rubrics organize *The Culture Factory*: Origins of Urban Schools; Society and Schools; The Machinery of Public Education; Segregation and Integration: Blacks in Public Schools; and Poverty, Immigration and Public Morality. Schultz's documentation is monumental with 70 pages of notes and references; there are 6 maps, 16 tables, and 12 reproductions of old prints. The author's theme, education for social change, is magnified by his choice of Boston for detailed inquiry; small (in the period subsumed by his title), virtually landlocked—an almost perfect laboratory. Yet it was this very insulation together with Boston's historic list of primacies in public education that now served to obstruct nineteenth-century European educational innovation and the fruits of pedagogical experimentation in neighboring states and in the new American West. Thus, little is said of the great educational reforms of Rousseau, Pestalozzi, and Froebel—the Prussian system as visited by Massachusetts's indestructible gadfly, Horace Mann, seemingly offering the greatest challenge to Bostonian pedagogical programing.

Schultz's topical arrangement makes for easier reading, but this reviewer thinks that a more thorough integration of the various currents hailed by Schultz as tributary to the Boston response to provision of a more perfect education system—since he has opted for a historical motif—could have importantly improved his account. But one could also easily infer that such a method results in the production of

Schultz's best contribution: first, that of the black education problem existing in the early half of the 1800s which includes a good exposition of the background of *Roberts v. City of Boston*, the fountainhead of "separate but equal" treatment of minorities and forerunner of the more famous *Plessy v. Ferguson*; second, that of the urban crisis occasioned by the influx within the Jacksonian period of "those hordes yearning to be free," especially by the vast Irish migration. For, regardless of sometime rioting, Boston never became a New World Belfast (chapters 9 and 10).

Since Schultz writes of education and society one wonders why he did not choose to enlarge his scope to encompass something of the vast social movement and innovative religions whose effect on the body politic is so well cataloged by Constance Rourke and Alice Felt Tyler. Surely these manifestations operated in the direction of education as well as in the socioeconomic sphere and are certainly as pertinent as the Know-Nothingism described in chapter 8. Furthermore one might, I think, properly quarrel with his title, the child of a presumed intuition rather than an objective statement of Boston's history. Nor does it appear that Schultz has mined the Bay newspapers as thoroughly as, say, Vera M. Butler in her *University of Pennsylvania* monograph.

Finally, while school buildings are described at great length, the same industry is not exhibited in a survey of the changing curricula over the seventy years considered, seemingly a highly pertinent fraction of the total maturation process. But before Schultz is accused of an antipathy toward public education or, at least, anti-educational administration, it must be admitted that of the two volumes described his is the much more scholarly endeavor, although James McLachlan's *American Boarding Schools* may be considered the better-rounded account.

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CHRISTOPHER MCKEE. *Edward Preble: A Naval Biography, 1761-1807*. Annapolis: Naval Institute Press. 1972. Pp. x, 394. \$16.00.

Little more remains in the American memory of the war with Tripoli in the early years of the nineteenth century than the second half of

the line from the Marine Corps hymn: "From the halls of Montezuma to the shores of Tripoli." The fact is that only a handful of Marines were involved. It was for the most part the Navy's war, and the leading figure in the conflict was Captain Edward Preble, commander of the Mediterranean Squadron in 1803 and 1804.

The salient facts about Captain Preble's life can be found in the large collection of family papers in the Library of Congress, Yale, and elsewhere, and Mr. McKee in his most recent biography of the captain has used this, as well as other material, to illuminate Preble's career. Edward Preble, third son of General Jedidah Preble, was born at Falmouth (now Portland), Maine, in August 1761. At sixteen he ran away to sea, serving for several years on a privateer. In 1780 he joined the Massachusetts navy, fought in several engagements, and was briefly a prisoner of war. With the return of peace he entered the merchant marine and in 1798, at the age of 37, was appointed lieutenant in the newly established United States Navy.

An important opportunity for him came in 1803, when Preble, as one of the most junior captains in the Navy, was given command of the Mediterranean Squadron during the war with Tripoli. Blockading Tripoli as well as he could with his tiny squadron (six vessels, the largest of which, the frigate *Philadelphia*, was captured and later burned), Preble assembled half a dozen gunboats and several other vessels. In August 1804 the small American force attacked the strongly defended North African port. Despite repeated assaults and heavy casualties, Preble was unable to take the town and soon after was relieved by Commodore Samuel Barron. Bitterly disappointed, he returned to the United States where Jefferson put him to work building gunboats for the Navy. In declining health, he finally returned home and in August 1807 passed away.

Preble's career in the young Navy, though brief, was important in establishing professional standards and in shaping a tradition that has served the Navy well. A hot-tempered, rough-tongued disciplinarian, he insisted on and received from his men the best that was in them. As commodore of the Mediterranean Squadron, he fashioned a professional fighting team. Many of his subordinates, later to become famous,

were then young men under thirty who came to be known as "Preble's boys," a title they carried proudly into the War of 1812 and beyond.

McKee has done full justice to Preble and to the history of the early years of the United States Navy. In doing so, he has added to our understanding of the role of the Massachusetts navy in the Revolution, the development of a professional corps of naval officers, and Jefferson's naval policy and strategy in the war with Tripoli.

LOUIS MORTON
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DONALD O. DEWEY. *Marshall versus Jefferson: The Political Background of Marbury v. Madison*. (Borzoi Series in United States Constitutional History.) New York: Alfred A. Knopf. 1970. Pp. x, 195. \$2.95.

MAURICE G. BAXTER. *The Steamboat Monopoly: Gibbons v. Ogden, 1824*. (Borzoi Series in United States Constitutional History.) New York: Alfred A. Knopf. 1972. Pp. vii, 146. \$2.95.

RICHARD C. CORTNER. *The Jones & Laughlin Case*. (Borzoi Series in United States Constitutional History.) New York: Alfred A. Knopf. 1970. Pp. ix, 191. \$2.95.

During the past few years the Alfred A. Knopf Company has sought to profit from college courses in constitutional history through the publication of a series of books focusing on pivotal Supreme Court decisions. At present three volumes have appeared dealing with the cases of *Marbury v. Madison*, *Gibbons v. Ogden*, and *National Labor Relations Board v. Jones & Laughlin Steel Corporation*. These books do not purport to offer fresh findings or revolutionary interpretations. Rather, they seek chiefly to synthesize past studies and present the student with a complete and accurate account of certain landmark cases.

Each of the works follows a standard format, beginning with a summary of the specific situation or controversy from which the case arose. Thus Donald O. Dewey's volume on *Marbury v. Madison* describes the midnight appointments of John Adams and the failure of Secretary of State John Marshall to deliver the appointees' commissions. Likewise Maurice Baxter's volume on *Gibbons v. Ogden* offers a de-

tailed narrative of the Livingston-Fulton steamboat monopoly, while Richard Cortner briefly portrays the origins of the National Labor Relations Board and the early antagonism felt toward that body. Each author then describes the judicial proceedings that arose from these controversies and summarizes the arguments of counsel. There next follows a detailed summary and analysis of the Supreme Court's decision. And lastly each author seeks to identify the impact or effect of the case on future constitutional development. In other words, they each present an accurate and detailed description of their specific case from beginning to end, from cause to consequence.

This narrow preoccupation with individual case histories, however, seriously limits the value of the Knopf series as an instrument for teaching constitutional history. The books in this series are not histories of legal or constitutional development but biopsies of "great" cases. Donald Dewey's work does not attempt to trace the origins and development of the doctrine of judicial review but rather the origins and development of the single case *Marbury v. Madison*. And in pursuing this purpose, he presents a detailed political history of the conflict between Jefferson and Marshall during the early nineteenth century. The history of this political controversy is the center of his attention and not the broader subject of judicial review. Moreover, this restricted focus often leads him to dwell on trivia while neglecting those vital factors that determined the evolution of constitutional doctrine. Thus while Dewey devotes four pages to the lives of those nonentities who acted as plaintiffs in the *Marbury* case, he dedicates not a single page to the political philosophy of balanced government that underlay the notion of judicial review.

Baxter and Cortner offer a somewhat fuller picture of constitutional development, devoting greater attention to the evolution of legal doctrine and less to the political machinations surrounding their specific cases. But again their volumes might have been more useful had each adopted a broader perspective and not limited his focus to a single landmark decision. For example, Baxter feels compelled to dwell heavily on the history of the steamboat because it was fundamental to the particular case he is examining. Yet he generally ignores other notable

changes in transportation and economic structure that might have equally influenced the Marshall Court's views on the federal commerce power. Likewise Cortner's volume would have been more helpful to the student if it had examined the constitutional development of the 1930s as a unit rather than singling out one case as the focus of attention. In general, then, the Knopf series suffers from a misguided preoccupation with the particulars of landmark decisions and as a consequence fails to describe adequately the broader factors influencing and determining the course of America's constitutional history.

JON TEAFORD

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RICHARD DRINNON. *White Savage: The Case of John Dunn Hunter*. New York: Schocken Books. 1972. Pp. xix, 282. \$12.50.

J. NORMAN HEARD. *White into Red: A Study of the Assimilation of White Persons Captured by Indians*. Metuchen, N.J.: Scarecrow Press. 1973. Pp. ix, 180. \$6.00.

CARL F. KLINCK and JAMES J. TALMAN, editors. *The Journal of Major John Norton, 1816*. (Publications of the Champlain Society, number 46.) Toronto: the Society. 1970. Pp. cxxiv, 391. By subscription.

The narratives of *White Savage*—marginal men and women who frequently preferred life in Indian society to being returned to a society and culture now alien to them—early attracted a wide readership here and abroad. For fifty years scholars have paid serious heed to captivities as documents of acculturation in cases of individuals caught between two cultures. During the Boasian emancipation from nineteenth-century racism, the late John R. Swanton first saw this genre as evidence opposing the doctrine of inherited psychological differences between races, and his brief paper in the *Journal of the Washington Academy of Sciences* (1926) is cited in two of these studies. Swanton's lead was followed by Erwin Ackerknecht, historian of medical anthropology, who interpreted the captivities as case histories in the enculturation of individuals (*Journal of the History of Medicine*, 15 [1944]: 15–36). Later historians, in the present instance, Richard Drinnon, and librarians, like J. Norman Heard, have found critical study of these documents

facilitated by the bibliographies of R. W. G. Vail (*The Voice of the Old Frontier* [1949]), the publications of the Newberry Library, and the work of Marius Barbeau for the American Philosophical Society (*Proceedings*, 94 [1950]: 522-48). The recent rage over the Indian as symbol for a literary image takes off from Roy Harvey Pearce's *The Savages of America* (1953). The three works under review owe something to each of these intellectual movements.

Drinnon's treatment of the case of John Hunter is most difficult to assess. There is only Hunter's memoir to go on, and it exhibits as remarkable an ability of its author to acquire literacy in a hurry as he purports to have demonstrated in learning the languages and assuming the cultural roles of the Kickapoo, Kansa, and Osage Indians on the southern plains. He had the advantage of being taken captive young enough so that he may well have learned the language (or languages), but he must have been something of a genius at that—as some observers seem to attest—to have later mastered literary forms so rapidly to have been able to publish his memoir by 1823—all within the span of two decades. In England he was introduced to royalty, held in high esteem by the gentry, vouched for in the Royal Society, and praised in literary circles. Like other prophets at home he was censured as an "impudent imposter" by Lewis Cass and Henry Rowe Schoolcraft, men who had a stake in the lands of the West and in Indian administration, which Hunter criticized; and his reputation for knowing the native languages was demolished by Duponceau. Catlin and others were more kind to him. Drinnon treats this controversy with circumspection. He writes in an engaging manner, and he has brought to his argument for Hunter complimentary evidence of some weight. For example, it is possible that Hunter could have learned to communicate in Osage without mastering its morphology and syntax and would therefore fail Duponceau's examination. But unlike Catlin, who gives a far better explanation of Plains culture, which he illustrated so nobly, Hunter pales as an ethnographer. This fascinating book does not really satisfy the issue of the authenticity of Hunter's memoir.

The question of how and why certain white captives rejected white civilization in favor of

"savage society" is examined by Heard on the basis of culture, family background, national origins, differential treatment in the Eastern woodlands and Plains west of the Mississippi, the time required to become an Indian, and especially the critical age of the captives when taken. He also examines the struggles, successes and failures to readjust, and as a control, looks at Indian children in white civilization. The treatment is systematic, always clear, and the documentation is superb. Among his cases is John Hunter, captured first in 1801 by Algonquian-speaking Kickapoos, treated miserably and lost by them to the Kansa—close linguistic relatives of the Siouan-speaking Osage—where he was adopted into a family in the place of a son killed by the Pawnees. Hunter spent fifteen years in captivity before escaping, and Heard estimates that he was 80 per cent assimilated. But he makes no comment on the authenticity of his memoir. Captivity early in life, before the age of puberty, seems to have been essential for learning the language and was detrimental to rehabilitation in white society. From an ethnographic point of view James Smith, who was eighteen when taken and spent four years among the Caughnawaga Mohawks in Ohio, and whom Heard estimates was but 20 per cent assimilated, made the most penetrating observations of Iroquois hunting techniques.

Major John Norton, not a captive, seems to have been the son of a Cherokee father and a frontier mother who was a Scot. Parts of his career are as obscure as his birth, but he lived principally among the Six Nations on the Grand River in Upper Canada—particularly among the Mohawks—serving as interpreter to the Indian Department. As the protégé of Joseph Brant, he was installed in the chiefly title of Teyoninhokarawen, a Mohawk form which in all the dialects means "It keeps the door open" and not what Klinck says it means on bad scholarly advice—namely, the title of the last office on "The Roll Call of the Iroquois Chiefs" (*Smithsonian Miscellaneous Collections* [Washington, 1950]). Indeed it is a pity that the otherwise excellent biographical introduction is marred by errors of an ethnological and linguistic nature that any of several Iroquoianists could have caught. Talman's historical introduction places Norton in his period.

The journal itself, written in England and long the property of the dukes of Northumberland, is especially valuable in reporting the Indian side of events on the Niagara frontier. It confirms the tradition of the settlement of the Six Nations on the Grand River after 1784; it contains a priceless sketch of the Deganawidah epic of the founding of their confederacy, which is paralleled by a contemporary version of Joseph Brant. Apparently Norton was Brant's literary heir and incorporated into his journal the traditional history that Brant had promised Samuel Kirkland he would write. This material is valuable up to a point, for it contains an Iroquois view of events as far back as the seventeenth century, but it also paraphrases much that was taken from books. Norton was a better reporter than John Hunter with whom he shared preferential treatment in England and Scotland.

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R. T. HUNTINGTON. *Hall's Breechloaders: John H. Hall's Invention and Development of a Breech-loading Rifle with Precision-Made Interchangeable Parts, and Its Introduction into the United States Service*. Edited by NANCY BAGBY. York, Pa.: George Shumway. 1972. Pp. xiv, 369. Cloth \$15.00, paper \$12.00.

More than anything else this is a book for the specialized arms collector. It is strongly based on the author's own collection and his wide acquaintance with other collections. Additionally, extensive use has been made of original reports and personal correspondence, which are preserved in the National Archives. The texts of some of the most important and lengthy of these are reprinted in the appendixes. Each chapter is followed by extensive footnotes.

John Hall is a very important figure in the evolution of the American system of interchangeable manufacture. He is substantially more important than Eli Whitney, who popularly receives all the credit. The book is not strong, however, in placing Hall's achievements in sharp focus vis-à-vis the accomplishments of Simeon North, Asa Waters, and a host of others, both here and abroad, who contributed increments, both before and after Hall, toward the eventual goal of close-limit interchangeability. The sig-

nificance of the Hall rifle as a vehicle for technological advance ends before 1830 when Hall's system reached its fullest development. The Simeon North armory in Middletown, Connecticut, later incorporated an extension of Hall's system, but other than this, Hall's work did not contribute to further advances.

For the collector of military arms and accouterments the book will probably always stand unchallenged and unsurpassed for the wealth of minute detail that has been painstakingly discovered, organized, and illustrated. Even the Confederate modifications and adaptations of obsolete Hall rifle parts, which were removed from the captured Harpers Ferry armory, are covered, as are the earlier civilian and experimental models.

This book will prove invaluable to those interested in identifying or collecting all the various models of Hall rifles and their accessories. For students of the evolution of manufacturing techniques and of interchangeable manufacture in particular, the book is a must, by far the most soundly based and complete point of departure that has appeared.

EDWIN A. BATTISON
Smithsonian Institution

MARK T. CARLETON. *Politics and Punishment: The History of the Louisiana State Penal System*. Baton Rouge: Louisiana State University Press. 1971. Pp. xii, 215. \$8.50.

GERALD N. GROB. *Mental Institutions in America: Social Policy to 1875*. New York: Free Press. 1973. Pp. xiii, 458. \$10.95.

Prisons and mental institutions, long an outward manifestation of an inward disgrace, excited the sympathy of humanitarians and provided an opportunity for malefactors to prey upon unfortunates who either flouted the law or, by no fault of their own, were unable to adjust to normal behavior patterns. Until recently, no attempt has been made to distinguish between the criminally sick and the mentally ill, so that, in far too many instances, each group has been "treated" with punishment by confinement in jails little different from asylums and asylums scarcely distinguishable from penitentiaries.

Competent studies of penal institutions and

mental hospitals, during any period of the nation's history, are scarce and many are less than objective. Administrators have cloaked their philosophies, their methods, and their discipline in secrecy. The hammer of research has failed to crack openings in institutional walls wide enough to more than peek through. Yet each attempt, no matter how small, widens the general vista. Both Carleton, in his study of the Louisiana state penal system, and Grob, in his discussion of the social policy of American mental institutions to 1875, provide material—although sometimes incomplete and marred by questionable generalization—that is sturdy enough to warrant publication and to lean upon.

Carleton's volume, which traces the changes, if not the progress, of Louisiana's varying methods of handling convicts from 1835 to 1968, seems to rest primarily upon the thesis or assumption that thoroughly materialistic motives were responsible for thwarting what he calls "enlightened" correctional policies. Although Carleton does not use the term "enlightened" in his text, this surely is what he means (pp. 7-8). What is even more difficult to believe, even though it might be true, is the connecting of racism with prison labor and reform. He lays heavy emphasis upon "this species of slave labor" during the decade from 1880 to 1890, maintaining that the desire to make money from convict labor was "so compelling and popular throughout Louisiana that the advocates of reform themselves were infected by it" (p. 42). This may answer one question, but it gives rise to another: Why, during the 1940s and 1950s, did the objective of the prison profit system remain unchanged? Why were the findings of professional investigations unchanged? Carleton, to his credit, lays bare the brutality, the political administrations ill-disposed toward penal reform, and the dehumanization of inmates. With equal objectivity he recounts attempts at reform. His concluding chapter, tinged with sadness and tintured with at least semirationalization, admits that prior to 1952 the state's penal system was essentially a business enterprise, administered "either by politicians or by lessees" (p. 192). He writes also that "as for public support of penal systems, the rest of the nation does not seem to be any more enthusiastic or reliable than Louisiana" (p. 198). It

would be most difficult to document that declaration.

Although the pattern of handling or rehabilitating convicts and lunatics was much the same for years, more change, if not progress, has been made in the care and treatment of the latter than the former, at least in many areas of the nation. Grob's sweeping survey, which in a sense could be regarded as a companion volume to Albert Deutsch's *The Mentally Ill in America*, not only is stimulating and provocative but also charts with an understanding objectivity and clarity the long, trying search by those who realized they had a problem to solve, but did not quite realize what their problem was. The author puts it in capsular form when he writes that to define the nature and causes of mental illness was only a beginning, not an end. "Psychiatrists then faced the question that had the greatest policy implication: what could be done to alleviate or cure mental disease?" (p. 165).

The dilemmas raised by that query, by the theory and practice of mental hospitals, and by the inability to distinguish between curable and incurable patients offer new and additional insights. Certainly the chapter discussing class, ethnicity, and race in mental hospitals is among the book's better sections. As in prisons, the author writes, some administrators "manifested unconscious hostility toward patients coming from backgrounds different from their own" (p. 222). Some promoted discriminatory practices, and others shared the racial and class prejudices of the larger society.

Not until after the 1850s did workers in the field of mental health realize that the mere founding of hospitals contributed little to problems posed by mental illness. From then until 1875 concerned individuals concentrated more and more upon the development of public policies, by which is meant the inclusion of mental hospitals as an integral part of political units such as states and counties. For this, legislation was necessary. Grob not only traces the difficulties and triumphs inherent in securing such legislation but also examines the structure and functions of institutions born of statutes throughout the country. The results, he points out, verged upon the tragic, for although participants in the mental health movement generally were "well intentioned" their "actual behavior

gave rise to less than desirable results" (p. 342). Like the prisons of which Carleton writes, "mental hospitals were not fundamentally dissimilar from most human institutions, the achievements of which usually fall far short of the hopes and aspirations of the individuals who founded and led them" (p. 342).

PHILIP D. JORDAN
Burlington, Iowa

DAVID M. PLETCHER. *The Diplomacy of Annexation: Texas, Oregon, and the Mexican War*. [Columbia:] University of Missouri Press. 1973. Pp. xiii, 656. \$20.00.

There has long been need for a fresh examination of the diplomacy of American expansion in the 1840s. A diplomatic history of the Tyler and Polk administrations has not been published since 1907. Scholars interested in the international aspects of the annexation of Texas and the Mexican War have had to rely on the outmoded and biased studies of Justin Smith, and analyses of the Oregon question have been scattered in scores of articles. David M. Pletcher, through intensive multiarchival research and the examination of nearly four hundred books and articles, many Mexican, has now provided a thorough reconsideration of the diplomatic aspects of these issues.

Pletcher's focus is on the origins and diplomacy of the Mexican War; however, he treats the Texas issue as a diplomatic problem in itself, as well as a cause of the war, and he examines the Oregon question as a collateral but separate consideration. On the diplomacy concerning the Texas and Oregon issues especially, he stresses the influences of economic and foreign political considerations, and he devotes considerable attention to the role of American politics throughout the book. Public opinion serves mainly as a background.

Pletcher's interpretation of the diplomacy of annexation is broad and provocative. Along with many other theses and revisions, he argues that American expansion generally, but particularly the annexation of Texas, created an international problem of major proportions; that America's diplomatic successes in the Oregon and Mexican War settlements occurred despite Polk's blundering; and that war with Mexico was neither necessary nor the best available means by which Polk could have satisfied

America's lingering continental aspirations. Much like Polk's contemporary critics, Pletcher is harshly critical of the president. Although Polk had limited goals in Oregon and wanted to acquire California he had no developed strategy for accomplishing these ends. His policies were expedient, politically-oriented, and lacking in direction. His tactics of bluff, bluster, and threat needlessly prolonged the Oregon issue and created the conditions that led to the war with Mexico. Skillful diplomacy and patience, Pletcher argues, would have led to the same American successes and would have served the United States better.

Pletcher's evaluation of Polk's diplomacy is well argued and supported, and his full, sensitive, and balanced discussion of Mexico's internal and foreign problems corrects the distortions of past scholarship. Unfortunately, although his treatment of the American side is superb, Pletcher has not considered as deeply the economic or ideological sources of European aims, policies, or purposes. His examination of the European role tends to diminish as he concentrates on the tortuous Mexican-American wartime diplomacy, thus weakening an important part of his thesis.

On the whole this is a fine book. Pletcher has challenged a number of recent interpretations (he denies that conspiracy played a part in Polk's program), and he has established an important new perspective to the historiography of American expansion in the 1840s. Whether or not one agrees with his interpretations and speculations Pletcher has provided an immensely valuable synthesis and has added considerable new material. This book supersedes all existing diplomatic studies of the problem and will long remain the standard text.

KINLEY J. BRAUER
University of Minnesota,
Minneapolis

LEONARD GILHOOLEY. *Contradiction and Dilemma: Orestes Brownson and the American Idea*. New York: Fordham University Press. 1972. Pp. xv, 231. \$10.00.

One of the reasons often advanced for the study of secondary figures in history is the insight they offer into the careers of more important persons and the movements of their time. Orestes Brownson's life should be of great in-

terest for this reason. He knew many of his more prominent contemporaries, particularly the Transcendentalists, and was part of the movement himself for a while. American scholarship could use a new and full biography of Brownson.

Regrettably, Professor Gilhooley has chosen to give us only a limited study of Brownson's thought. The format of the book is a summary of the articles Brownson wrote between 1838 and 1859. The result is rather choppy, superficial, and repetitious. The author never gets below the surface of what Brownson says and never really traces the development of his thought because we do not see what causes him to think as he does.

What does come through is a picture of Brownson as a man of incredible vanity and conceit who always tried to show how much more he knew than everyone else. Theodore Parker, Ralph Waldo Emerson, Edgar Quinet, Jules Michelet, and George Bancroft, to mention only a few, were all demolished by his brilliance and learning, or so he thought. Posterity has decided otherwise. Parker and Emerson are in the mainstream of American thought. Michelet and Quinet still rank as great scholars. Bancroft's significance in American historiography is secure.

Evidently something was eating Brownson, and until we find what it was, we cannot really understand his thought processes. His conversion to Catholicism was the focal point of his life. For the rest of his career he was venomous toward his former associates among the Transcendentalists, in particular Parker. Brownson never missed a chance to denounce Protestantism as inimical to American democracy, while insisting that the nation could not survive without Catholicism. As Gilhooley unwittingly shows, however, Brownson did not believe in democracy or religious freedom, but grew steadily more bigoted as he aged. Regrettably the author has omitted Brownson's discussion of the Mortara case, which shows how little regard Brownson had for the rights of others.

Perhaps sometime someone will produce a significant book on Brownson, based on real research. It should be fascinating.

HAROLD SCHWARTZ
Kent State University

KATHRYN KISH SKLAR. *Catharine Beecher: A Study in American Domesticity*. New Haven: Yale University Press. 1973. Pp. xv, 356. \$12.50.

The accomplishments of the eldest child of Lyman Beecher have for too long been overshadowed by the fame of her younger siblings, Henry Ward Beecher and Harriet Beecher Stowe. After surviving the perils of being Lyman's daughter and the trauma of the death of a fiancé selected by her father, Catharine Beecher (1800-78) became an expert in domestic economy, an advocate of moral education for children, the founder of numerous seminaries to train young women to become teachers, and the author of religious and philosophical tracts. Her most important work, *Treatise on Domestic Economy* (1843), "simplified and made understandable the mysterious arts of household maintenance, child rearing, gardening, cooking, cleaning, doctoring, and the dozen other responsibilities middle class women assumed" in the nineteenth century (p. 152). It also made her name a household word in the 1840s. Catharine Beecher herself never had a home or children, disdained teaching, and consistently believed that her personal life was one of self-sacrifice. Her moments of greatest pleasure seem to have occurred when she visited water-therapy establishments for "the cure," which provided women a socially acceptable opportunity to indulge in sensuality and to discuss frankly their own bodies and their attitudes toward men.

The strength of Mrs. Sklar's biography lies in her analysis of the intellectual development of her subject. Catharine Beecher believed in common sense, the submission of the self to the general good, the possibility of women dominating social change by expertly managing the home front, and the moral superiority of the traditional submissive role of women. One of America's pioneering Victorians, she believed "the protection of knowledge" should govern relations between the sexes. The tenets of Calvinism remained central to her thought; she insisted, however, that God saved not only those who had been convicted of sin but all who wanted to be saved. Both she and Harriet attempted to translate "Calvinism into a social rather than a religious system" (p. 242).

Despite many subtle revelations of Catharine Beecher's humanity and occasional references

to her "deep psychological problems" (p. 169), the author has refrained from the kind of penetrating analysis that might have made more interesting reading about a woman whose life, except for her visits to the water cure, was usually unexciting. At the same time the author has avoided the pitfalls of historians who use too hastily a discipline in which they are not well trained. The facts are skillfully enough collected and presented to allow the reader to draw his own conclusions. *Catharine Beecher* is a welcome addition to the literature about the famous Beecher family and a notable contribution to our understanding of the social and intellectual development of the United States.

E. STANLY GODBOLD, JR.
Valdosta State College

JON L. WAKELYN. *The Politics of a Literary Man: William Gilmore Simms*. (Contributions in American Studies, number 5.) Westport, Conn.: Greenwood Press. 1973. Pp. xiv, 306. \$10.00.

Professor Wakelyn's objective "is to illustrate Simms' social and political values as revealed in his fiction and to show the slow shift in the views of this young intellectual seeking maturity in the South." Interpreters of Simms have generally classified him as a potentially gifted writer neglected by his social superiors whose cause he served or as a man who dissipated his literary energies trying to prop up the Southern Muse. Wakelyn maintains that Simms became a man of influence in his state and the South and that politics—rather than diverting him from literature—enriched his work. For him, "literature and politics were always combined, never separated." Fiction and historical writing were vehicles for his political and social theorizing.

Although Simms began as a nationalist and an adherent of his state's Unionist faction, his social philosophy remained conservative. From the 1830s on, he defended slavery even while he attacked nullification or held forth on the dangers of Western expansionism. Sectional strife turned him into a strident secessionist as well as affectionate critic of the South. He scolded the planters' narrow agricultural bias and destructive agronomy, worried in fiction and nonfiction about divisive tensions within

his section, and worked constantly to undermine the South's old political allegiances in favor of a great disunion party.

Wakelyn painstakingly documents Simms's public career and his passionate defense of his state. The evidence he presents for Simms's political influence is less persuasive. Whose ideas did he change, whose "dormant minds" arouse? Why did his publishing ventures invariably fail? Wakelyn gives us a fresh view of the writer-politician, but his political analysis of Simms's fiction and occasional comments on Simms's ambivalent response to Southern "leadership" suggest a personality more complicated and contradictory than the well-adjusted patriot of his book.

DANIEL AARON
Harvard University

NEAL C. GILLESPIE. *The Collapse of Orthodoxy: The Intellectual Ordeal of George Frederick Holmes*. Charlottesville: University Press of Virginia. 1972. Pp. x, 273. \$9.50.

George Frederick Holmes (1820–97) enjoyed considerable prominence during his lifetime as an author, social critic, and educator. Although scholars have written about him previously, this is the first full-length biography to appear. Based on Holmes's private papers and his extensive publications as well as on other pertinent materials, this book provides a complete and balanced account of the man and his achievement.

Choosing his perspective wisely, the author uses the intellectual ordeal of Holmes to illuminate the revolution in thought that was occurring in the nineteenth century. Professor Gillespie skillfully combines the story of a life and of a mind. An ambitious and condescending Englishman with one year at Durham University, Holmes arrived in the South in 1838, living there until his death without ever becoming a United States citizen. He married into a prominent Virginia family and aspired toward the law but quickly turned to writing and teaching. The choice brought years of privation and disappointment before he finally landed a permanent professorship at the University of Virginia, but it enabled him to grapple seriously with ideas.

Three interrelated issues dominated his

thought. The problem of faith and reason was basic. Despite a period of religious infidelity, the reaction against which is never adequately explained, Holmes struggled throughout his life to reconcile Christian faith with contemporary science and historical scholarship. Honest doubts triumphed during his last years. Closely related to his religious crisis was a search for the laws of human society, which led him to Aristotle, Bacon, and Comte, but not, significantly, to contemporary German thought. Although he remained critical of Comtean Positivism, Holmes helped introduce the philosophy to America. This book supersedes earlier accounts of Holmes and the origins of the science of society in the United States, though it stresses the historian at the expense of the sociologist in the man. Last was Holmes's defense of the civilization of the Old South and apology for slavery, to which the author imparts freshness by treating the subject within the total configuration of Holmes's thought. Taking a lead from Eugene Genovese, Gillespie views Holmes as a bourgeois defender of slavery.

The Collapse of Orthodoxy, which began as a doctoral dissertation, is in all important respects a credit to its author.

WINTON U. SOLBERG
University of Illinois,
Urbana-Champaign

NORMAN C. DELANEY. *John McIntosh Kell of the Raider Alabama*. University: University of Alabama Press. 1973. Pp. 270. \$8.50.

John McIntosh Kell, executive officer of the ill-fated Confederate raiders *Sumter* and *Alabama*, was an aristocratic, vain, handsome, and self-satisfied Georgian whose role in American naval history has been largely obscured by his shipboard associations with the legendary Raphael Semmes.

This compact biography brings Kell out of the shadow of the towering Semmes and establishes him in his own right as an officer of enormous technical skill and considerable tactical insight. A man of great personal bravery in combat, he was also professional and sensible enough to have advised his commander not to fight the superior *Kearsarge* on that sunny day in June 1864. Indeed, Delaney has given us the most realistic (and demythologized) account

ever written of the long and successful cruise of the *Alabama* and the brief, historic, and foolhardy fight off Cherbourg that ended Confederate naval pretensions. Semmes's later rationalization that he had somehow been tricked into fighting a camouflaged ironclad (or chainclad) was never particularly convincing. Nor is Delaney convinced.

Like most American career naval officers of the mid-nineteenth century Kell had little political sophistication and less social sensitivity. He really had no idea what the Mexican and Civil Wars were all about, although he saw combat in both. He accompanied the Perry expedition to Japan without in any way understanding its larger purpose or significance. His sudden conversion to the Southern cause in 1861 was an automatic reflex rather than a reasoned decision.

Coupled with Kell's sophomoric political opinions was a deep sense of personal honor and an abiding faith in the supposed prerogatives of gentlemen. This last was so exaggerated as to cause him to be court-martialed (for failure to obey a direct order he considered demeaning) and briefly dropped from the service in 1849. Not surprisingly, his attitude toward the polyglot crew that manned the unhappy *Alabama* bordered on the contemptuous: "A sailor is a sailor. He has few attachments, and as a general thing doesn't care what flag he is under."

Nor did he or Semmes have high regard for the brain power of those ersatz Confederates, those "liars, thieves, and drunkards," who were shot to bloody bits by the far more efficiently served guns of the *Kearsarge*. "My crew were never so happy as when they had plenty to do, and but little to think about. Indeed, as to thinking, I allowed them to do very little of that," Semmes later boasted. This view was shared by Kell and is one that has not changed much among American naval officers since the Civil War.

Delaney has relatively little specific Kell manuscript material with which to work. He has, however, thoroughly mined the primary and secondary sources near and around Kell and has skillfully pieced together a variety of related family letters, printed memoirs, and official records. Included among these are the postwar recollections of Kell himself and those who had fought with and against him. The

result is a wholly satisfying book that belongs on the shelf of every American naval historian and every Civil War buff.

ROBERT SEAGER II
University of Baltimore

THOMAS LAWRENCE CONNELLY. *Autumn of Glory: The Army of Tennessee, 1862-1865*. Baton Rouge: Louisiana State University Press. 1971. Pp. ix, 558. \$15.00.

In *Autumn of Glory* Thomas L. Connelly completes his monumental two-volume study of the Civil War in the West. In well-written narrative he follows the Army of Tennessee from the renewed offensive of General Braxton Bragg, which began near Murfreesboro, Tennessee, in late 1862, to the lingering days of retreat and defeat in North Carolina in 1865. With meticulous research and careful interpretation, he traces this army's actions as it dramatically defends but loses mile after mile in battle after dreary battle the "heartland" of the South—thus sealing the Confederacy's doom.

Connelly milled much of this work as a labor of love from his summer home in the midst of the heartland, and his descriptions come from firsthand knowledge of the region. In his occasional lightning flashes of genius, he brings the war to life—in the mountains, beside the rivers, and in the towns and cities of Tennessee and Georgia. His scenes of involvement extend from the planning table, through the march, and into the blood and death of the battlefield. The intent reader cannot but remember the vivid pictures of the battle of Franklin, the campaign for Atlanta, or the siege at Chattanooga.

Though he pictures the battles in intimate detail, the author is at his best in analyzing the men in command. He excels at stripping the leaders of their glory, and through his pages generals lose much of the glamour that has grown up around them. His penetrating evaluations confirm my own belief that generals lost as many battles as they won, their victories achieved more by indecision and mistakes in the enemy command than by genius in their own. Connelly's generals also usually appear as insensitive, even callous, to some of the larger issues of war that brought them to command in the first place and to the men whose

lives they too freely squandered in their efforts to win victories and glory.

Connelly is critical of the actions of leaders on every level of command, including those in power in Richmond whose decision making often left the West to its own dwindling resources to protect a region as integral to East as to West. In Richmond it is Jefferson Davis who is the prime target for criticism, but Robert E. Lee also falls in the onslaught. Sometimes the Virginian appears as indecisive, as in the summer of 1864 in his unwillingness to make a firm commitment on John B. Hood's appointment to high command; as uncooperative, as in the summer campaign of 1863 when he refused to consider a larger concerted defense to relieve the siege of Vicksburg; or as possessing narrow vision, as both he and the Confederate president failed to comprehend the realities and importance of the war beyond the Virginia theater.

In the Army of Tennessee Bragg and Joseph E. Johnston drew heavy criticism, but the subordinate generals, including William J. Hardee, John Hood, and Leonidas K. Polk, also feel the author's wrath. In analyzing Bragg's command, Connelly weaves through the intricate patterns of intrigue and confusion in the bizarre events surrounding that general's incompetency before William Rosecrans, especially at Chickamauga, and before Grant at Chattanooga. Bragg, an "irritable and impatient" man, possessed of "an amazing sense of poor timing," presents his worst side in reacting to the round robin from his generals that threatened his removal from command. Johnston, always seeking greater personal recognition, fares little better in the author's judgment: for his unwillingness to accept responsibility at Atlanta, in the struggle between Bragg and his generals, and in other assignments in the West and for his personal dislike of the Confederate president, which affected his decision making. Hood, though courageous and loyal, is pictured as a near incompetent in his leadership at the fiascoes at Atlanta, Franklin, and Nashville.

Among the "lieutenants," Thomas C. Hindman shows "a remarkable disinterest in taking the offensive"; Breckinridge is drunk on the field of battle; and Polk stalls when ordered to make a direct attack on the Federal position at Rock Spring Church.

Among the war's leaders, human weaknesses too often prevailed, and through their carping correspondence, the memoirs of those with whom they fought, and the official records and reports, Connelly lets these men seal their own fates. Though he recognizes the precariousness of the Confederacy in its bid for power before the superior strength of the federal government, Connelly sees internal discord among these commanders, the intricacy of Confederate command decisions, and the absence of decisive decision making by these commanders as important as the limited resources and manpower and dwindling supplies in contributing to defeat.

Autumn of Glory is a study of the Civil War that subjects men and events to close scrutiny and then pronounces judgment upon them. This is a rare quality in writing, especially when backed by intense research, a sharpness of interpretation, an effective writing style, and an uncanny depth of perception of human nature. This book is destined to join the library of Civil War military classics along with Douglas Southall Freeman's *Robert E. Lee* and *Lee's Lieutenants*, Bruce Catton's trilogy on the Army of the Potomac and his *Centennial History of the Civil War*, and Kenneth P. Williams's *Lincoln Finds a General*. It may be equaled by historians writing about the Civil War in the West, but it will not be surpassed.

ROBERT HARTJE
Wittenberg University

ROBERT F. DURDEN. *The Gray and the Black: The Confederate Debate on Emancipation*. Baton Rouge: Louisiana State University Press. 1972. Pp. xi, 305. \$10.95.

Several scholars, as Robert F. Durden points out, have published studies of the discussions among Confederates in the winter of 1864-65 that culminated in the enactment in March 1865 of the law providing for the enlistment of slaves in the Confederate army. But Durden maintains in *The Gray and the Black* that those discussions were part of a larger debate among Confederates over whether their government should adopt a policy of emancipation. Durden's book describes that larger debate over emancipation by reprinting portions of it in chronological order as it was carried on in newspaper

editorials, letters, addresses, and other such sources. He has encased those materials from the 1860s in a running narrative commentary written by himself, and to this he has added a seven-page "Note on the Historiography of the Problem."

The Confederate debate over emancipation as described by Durden was motivated primarily not by humanitarian considerations but by the prospect of military defeat. The goal of those Confederates who urged emancipation was to increase the manpower in the Confederate armies and/or to secure the support of European nations; their rationale was that winning Confederate independence took precedence over preserving slavery. The highest ranking Confederate who advocated emancipation was the Confederate president himself, and the major focus of *The Gray and the Black* is upon Jefferson Davis and his message to his Congress of November 7, 1864. In that message Davis recommended the purchase by the Confederate government of 40,000 slaves, who would be used as laborers with the Confederate army, and who would be promised their freedom on "discharge after service faithfully rendered."

Durden has performed a service of great value in reminding us that Confederates debated emancipation among themselves and in describing and analyzing that debate more completely (so far as I am aware) than any previous scholar. My guess is that all readers of *The Gray and the Black*, even those readers who have studied the Confederacy, will learn much from it. Moreover, Durden makes illuminating comparisons between the Confederate experience and that of the Union, as well as many thoughtful comments on the significance and meaning of what he describes.

The Gray and the Black contains generalizations about opinions of Confederates concerning emancipation that raise difficult problems of evidence. The advocates of emancipation are described as a minority, opposed by the great mass of Confederates. But the large majority of the selections reprinted in the book were written either by high ranking Confederate officials or by newspaper editors. These selections may constitute evidence of the views of "leaders" or of the "elite public," but not necessarily of the mass of white and black Southerners. In fair-

ness to Durden, he has simply followed the practice of most historians (myself included) in generalizing about mass public opinion when his evidence pertains chiefly to "leaders." I think that historians (myself included) could and should be more precise and accurate in generalizing about opinions, and I regret this weakness in an otherwise valuable book.

THOMAS J. PRESSLY
University of Washington

ROLLIN G. OSTERWEIS. *The Myth of the Lost Cause, 1865-1900*. [Hamden, Conn.:] Archon Books. 1973. Pp. xii, 188. \$8.50.

In this slim volume Professor Osterweis returns to a theme sounded in his earlier work, *Romanticism and Nationalism in the Old South* (1949). He here rounds out the analysis by an examination of the persistence of antebellum Southern romanticism into the post-1865 era in the form of the myth of the Lost Cause. In eleven closely written and largely self-contained chapters he sketches the emergence of the myth in the aftermath of defeat; its institutional expression in the original Ku Klux Klan, the United Confederate Veterans, and the United Daughters of the Confederacy; its literary evocation by Southern regionalist writers, poets, reports, and orators; its dissemination to Northern readers by circulation-hungry editors of national periodicals; and its ultimate triumph on Northern stages, in Northern and Southern textbooks and classrooms, and in segregated Southern churches. An epilogue traces the checkered career of the myth through the vicissitudes of the twentieth century into the social turmoil of the 1960s.

Methodologically, Professor Osterweis has relied on a concept of myth articulated by Ernst Cassirer and Susanne K. Langer. "A myth," he has noted, "usually has only a shadowy basis in empirical fact; its power depends upon the effectiveness with which it displays in symbolic form the value aspirations of a people" (p. x). Given this orientation, the selection of evidence tends to be impressionistic or illustrative; younger historians with quantitative, behavioral, or social structural biases will be dismayed at the lack of rigorous methods of proof and by the hypostatization of explanatory concepts.

Chronologically this work reflects the presuppositions of the debate on the Reconstruction era as it stood in the late 1940s and early 1950s. The updating of sources in the footnotes seldom is reflected in substantive changes in the main text of the argument. Given these limitations of method and scope, the book is still important for the questions that it raises and attempts to answer: Why has a romantic myth persisted over time, in spite of the destruction of the social setting that gave it birth? How can the intellectual historian take into account the arational, emotive symbols that shape the content of popular thought? Why were whites in the North so willing to accept the myth?

ROSS EVANS PAULSON
Augustana College,
Rock Island

MARTIN E. MANTELL. *Johnson, Grant, and the Politics of Reconstruction*. New York: Columbia University Press. 1973. Pp. 209. \$9.00.

ERNEST MCKAY. *Henry Wilson: Practical Radical. A Portrait of a Politician*. (National University Publications, Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1971. Pp. 262. \$11.00.

RICHARD H. ABBOTT. *Cobbler in Congress: The Life of Henry Wilson, 1812-1875*. [Lexington:] University Press of Kentucky. 1972. Pp. xii, 289. \$13.50.

To the already considerable body of scholarly works on the political history of Reconstruction we may now add three more. *Johnson, Grant, and the Politics of Reconstruction*, a revised version of the author's doctoral dissertation, purports to be a fresh examination of the election of 1868 and the role of Ulysses S. Grant in Radical Reconstruction. Unlike most recent historians of the Radicals, Mantell downplays their moral platform and characterizes the Republicans in general as shrewd politicians who enacted what legislation they could and then retrenched themselves in power by picking a sure-fire presidential winner in 1868. He also suggests that bonds and greenbacks were issues of as much importance as Negro suffrage in the Midwestern congressional elections of 1867. But none of this is entirely new, nor does Mr. Mantell really fulfill his promise to shed fresh light on Grant as a politician.

While the general's actions and statements in 1866 and 1867 may be interpreted, as they are by Mantell, to favor the Radicals, the author has not done very much to enlighten us as to why Grant acted the way he did. Grant's political motives and sympathies remain an enigma. The book has all the ponderous earmarks of a dissertation, replete with obvious statements like this one: "In summary, the northern political system showed the overriding influence of the war" (p. 147). The footnotes contain an impressive array of primary sources, but most of these materials have been thoroughly digested elsewhere. This is the kind of thesis that should have been presented to the profession in two or three concise articles.

Henry Wilson has long stood in need of a biographer, and now he has two. The junior senator from Massachusetts might very well serve as the archetype of a Civil War Republican, but neither author has explored this possibility fully. Wilson was born in poverty in a small New England town and spent his boyhood and youth as an indentured servant. From these humble beginnings he went on to learn to read and write and eventually acquired a small shoemaking business. The "Natick Cobbler" then challenged the political hegemony of the first families of Massachusetts in his attempt to ride the crest of a new antislavery party into government. Wilson's life and views affirm in microcosm the ideology Eric Foner attributes to antislavery Northerners in his recent study of the Republican party in 1850s. Wilson's self-improvement was evidence of the harmonious interests of worker and businessman, the value of the Protestant ethic, and the reality of social mobility. He argued from the 1840s that an insidious "slave power" governed in Washington and that slavery would collapse if the federal government withdrew its support from the institution. At the same time, Wilson seems to have been less racist than the average Republican; his own servitude gave him a lasting empathy with blacks in bondage. He had consistently supported education for Afro-Americans and an end to discrimination and segregation long before such measures became official Republican policy.

The Republican party was primarily developed to serve as a political vehicle for these values, but Republicans had to be as ruthless

as other politicians to enact their point of view. Unlike so many biographers of the Radicals neither McKay nor Abbott belabor the idea that Wilson was inconsistent because he was both an idealist and a consummate politician. He was entirely capable of disguising his most radical opinions if the populace was not yet ready for them. He often sought to disassociate himself from the more radical Abolitionists and temporarily became a Know-Nothing in the 1850s when nativist sentiment hit its peak in Massachusetts. No other politician was more responsible for building a Republican organization in Massachusetts. During the Civil War he became chairman of the Senate Committee on Military Affairs and was instrumental in drafting the conscription bill that allowed draftees to escape military service by paying three hundred dollars or providing a substitute. He promoted bounties to attract volunteers and used his influence to assist Governor Andrews in obtaining black recruits in the South to fill Massachusetts quotas, thus avoiding the drafting of needed factory workers. Like most Republicans Wilson thought it best to devise policies that would promote productivity (keep workers working) and harness blacks to fight for their own freedom. Wilson consistently aligned himself with other Radical Republicans during Reconstruction, and in 1872 the party rewarded his long service by making him the vice presidential nominee. Wilson had long since given up his shoe business and had lived modestly on his political salaries; he had to borrow money from Charles Sumner to buy a suit of clothes for the inauguration. He died in office in 1875.

These are traditional political biographies and are based on almost the same primary materials. Neither author has used secondary sources to any great extent or done as much as he might to put to use the work of Eric Foner, John and LaWanda Cox, W. R. Brock, or David Montgomery. Both amply tell the story of Wilson's life, although Mr. Abbott's writing is the more precise and lucid.

Students of Reconstruction have been overwhelmingly interested in political history and these three authors are no exception. But as Willie Lee Rose and David Montgomery suggested some time ago the real action may have taken place elsewhere. These books round out

our political knowledge, but they do nothing to break new ground in the social and economic history of Reconstruction.

SHARON HARTMAN STROM
University of Rhode Island

JIMMY M. SKAGGS. *The Cattle-Trailing Industry: Between Supply and Demand, 1866-1890*. Lawrence: University Press of Kansas. 1973. Pp. ix, 173. \$8.00.

This study is illustrative of the current reaction of historians to the romanticized views of Western economic enterprise, a new emphasis upon institutions rather than biography. Recent studies of the fur trade, mining, and the range-cattle industry have illustrated the ways in which American business objectives and methods were practiced on the frontier as well as in Eastern urban centers. Slowly but surely the frontier experience is being integrated into the economic history of the United States, and its impact on foreign trade and international relations in the nineteenth century is being made apparent. Jimmy M. Skaggs is specifically concerned with cattle-trailing contractors, entrepreneurs who seized the opportunity for profits from delivering vast herds of Texas cattle to the Northern market. They were typical businessmen in the Gilded Age. The importance of this little-known group of middlemen is appreciated when one recognizes that not more than ten to fifteen percent of all cattle moved to market were handled by the lionized cattle barons who raised the livestock. That job was handled by contractors, "hip pocket businessmen," who left few records, but Skaggs has located and analyzed evidence on twenty-two men either working alone, in partnerships, or with a family enterprise. The dominant characteristic of the group was the ability to improvise and innovate, thus no two contracting groups functioned the same way, nor did individual contractors find it expedient, apparently, to try the same scheme twice. Some were financially successful enough to become cattle barons, real estate promoters, or bankers in later years. Both the railroads and the Midwestern slaughterhouse towns reaped an economic benefit from the work of trail contractors. Had their enterprising genius and innovative skills extended to establishing large

meat-packing companies in Texas and the rail facilities to ship eastward, their significance in the nation's history would have been even greater. Skaggs suggests that the cattle-producing West and Southwest were colonial outposts of the East in a "mercantile framework," and speculates on the impact to the economy of the Western states had the meat-packing industry been centered there.

This is not a pretentious book. Four of the eight chapters, probably the best, have been previously published as articles in regional journals. The additional material, however, fills important gaps, achieves unity in handling the topic, and provides interested readers easier access to the material. Skaggs has performed a service in focusing the attention of historians on an important but neglected aspect of the cattle business.

W. TURRENTINE JACKSON
University of California,
Davis

TED C. HINCKLEY. *The Americanization of Alaska, 1867-1897*. Palo Alto, Calif.: Pacific Books. 1972. Pp. 285. \$8.95.

DAVID WHARTON. *The Alaska Gold Rush*. Bloomington: Indiana University Press. 1972. Pp. xiii, 302. \$8.95.

Ted C. Hinckley has produced a masterfully written, thoroughly scholarly book. *The Americanization of Alaska* is essential reading for anyone concerned about the Far North's development. Hinckley's study is both an intelligent reconstruction of the past and a contemporary document, contemporary because it concerns Alaska's current pressing problems: native acculturation, the clash between conservationists and exploiters, a population overwhelmingly urban despite all the breast beating about life in the wilds, and a precariously narrow economic base composed of absentee-controlled natural resources.

These problems existed from the United States purchase of the Great Land in 1867, through the abandonment of military rule ten years later, through the first Organic Act of 1884 making Alaska officially a territory, and through the opening scenes of the Klondike stampede of 1897-98. The issues were fought out mostly on the Alexander Archipelago and the south-

eastern littoral against a backdrop of silvery green, spruce-covered hills. Hinckley argues conclusively that the southeastern panhandle had passed beyond its uncertain pioneer stage by 1897 and was ready for separate statehood. Its stability rested, he finds, upon a tripod of effective government, economic development (sealing, fisheries, and gold), and social maturation. While pursuing his argument the author chronicles a good many setbacks such as Sitka's wilting in the postpurchase years. He paints a number of lively, warts-on portraits, including those of the dynamic Presbyterian missionary Sheldon Jackson and two of Jackson's early antagonists, Governors John H. Kinkead (1884-85) and Alfred P. Swineford (1885-89).

Hinckley convincingly demonstrates the differences between frontiers in Alaska and in the contiguous West. Alaska was irrelevant to Reconstruction and post-Reconstruction concerns. Its frontier was urban or, in those days, town centered. Alaska was far away from the rest of the country, and its white population was numerically insignificant. These circumstances delayed its rise to full territorial status. Hinckley's sympathetic but firm rejection of the inevitable sourdough wails about federal "neglect" places him in the revisionist school of Alaska historians.

What was understandable in the nineteenth century is not acceptable in the twentieth. Hinckley closes with the warning that Alaska's survival as a wilderness area requires careful policy decisions. The policy makers, and their staffs, could begin by reading this book.

David B. Wharton's *The Alaska Gold Rush* takes up where Hinckley leaves off, but only in a chronological sense. Wharton recounts the human drama of gold stampedes in the Canadian Yukon and in Alaska west of the panhandle. His is a good popular history. He writes well without indulging in cloying purple passages. He has traveled the old trails, visited the decayed mining camps, and has done more archival research than most writers of the genre. Yet his book falls short of reaching the standards represented by Hinckley's study.

The nonspecialist may glean some entertaining, useful information from *The Alaska Gold Rush*. Wharton, however, deprives his reader when he fails to provide anything like the con-

ceptual framework guiding Hinckley's equally facile pen. What about the political impact of the Alaska gold discoveries? Wharton is mute. What about the extension of federal activity northward? Army officers, telegraph building, and road construction are mentioned, but not related to the growing bureaucratic effort in the territory. What about native acculturation? Wharton limns a stark, oversimplified portrait: a few traders and missionaries befriended the natives, but other whites were mean.

Wharton's paean to Alaskan individualism strikes a false note. He parades all the well-known characters—"Soapy" Smith, Jack McQueston, Trader Bean, "Tex" Rickard—and others less well known. Then there are the women—the unsung heroines of the trail who worked diligently alongside their men. Or may we now call them "sung" heroines, for their exploits have been chronicled so often? The individualism in all of this, if individualism is the name for it, was incidental and often bizarre. Most stampedeers did their work for companies or under other cooperative arrangements. The gold seekers reconstructed an elaborate community life when they arrived in the Far North, an effort Wharton depicts only to deny. Frequently he confuses struggle and hardship with individualism.

Wharton is concerned with giving his reader the "truth" and the full story, but his narration of the Nome claim jumping is partial and simplistic. His discussion of Valdez railroad developments is partisan and erroneous. The general reader to whom Wharton's book is addressed probably will enjoy the sourdough tales and profit from some of Wharton's judgments about the last stampede. If he has time for but one of the books, however, he should join the specialist in reading Hinckley's.

WILLIAM H. WILSON

North Texas State University

GERALD ROSENBLUM. *Immigrant Workers: Their Impact on American Labor Radicalism*. New York: Basic Books. 1973. Pp. vi, 189. \$8.95.

Why has the United States not generated a sustained radical working-class movement? Professor Rosenblum, a sociologist, brings to bear upon this historical problem the theories of modernization. His answer in brief is that dur-

ing the most intensive phase of industrialization (1880-1910) a massive immigration muted the "strains of discontinuity" thus forestalling the mobilization of labor requisite for its radicalization. The "new immigrants" of peasant origin became "expedient members" of American society related to it only by a "cash nexus." Lacking basic socialization in American values, retaining a primary orientation to their homeland, and living in insulated "ethnic subsystems," they did not experience the changes wrought by modernization as discontinuities. Willing to accept wage labor and technological innovations, the immigrants were "poor recruits for radical activity." Rosenblum recognizes that these same immigrants often participated in union activities and labor struggles. But he interprets this involvement, even in the IWW-led strikes of Paterson and Lawrence, as an expression of conservative "job consciousness" rather than revolutionary class consciousness. Thus the immigrant workers are viewed as largely responsible for the "unique characteristic" of the American labor movement: its business unionism.

Native workers, on the other hand, as "genuine members" of American society, were mobilized and radicalized by the contradictions between values and actualities arising from modernization. They and those immigrants coming from industrial backgrounds (Germans and East European Jews) were the source of all labor radicalism. In this scheme the Knights of Labor, the American Socialist party, the Western Federation of Miners, and the IWW in its more extreme form are interpreted as manifestations of the "strains" experienced by native workers in the course of modernization. However, the movement of acquiescent foreigners into the most exploited jobs and the rise of the natives to more favorable positions curbed this radical response.

In my judgment the author has substituted a myth of native radicalism for the myth of immigrant radicalism. Rosenblum's conclusions flow from his initial assumption that native Americans and new immigrants comprised ideologically homogeneous and contrasting entities. Such a dualism, however, can not stand the test of empirical evidence. If native workers responded radically to the strains of modernization how then does one explain the lack of a

radical labor movement among Southern textile workers? If East European peasants were immune to radicalism how then does one explain the socialism-IWWism-communism of the Finns? A pluralistic model that would comprehend the broad spectrum of experimental-ideological sets which the industrial recruits, both native and foreign born, brought to the factory would, it is suggested, provide a more fruitful strategy for future research.

Although Rosenblum asserts that this essay is "not intended as single-factor theorizing but, rather, theorizing about a single factor," his argument has a dogmatic tone about it. Other explanations of the conservatism of American labor are dismissed rather summarily. While the study draws upon statistical sources and monographs for supporting data, one has the feeling that evidence which does not support the argument is either ignored or argued away. The author's familiarity with the relevant historical literature appears to be somewhat spotty. Among the historians whose works are not cited are Herbert Gutman and Victor Greene. However, Rosenblum should not be held responsible for the deficiencies of the historical literature upon which he does rely. The fact is that the labor movements among Eastern and Southern European groups (excepting the Jews) have been little studied by American historians.

Rosenblum deliberately formulated his argument in bold terms so, as he says, to provide critics with a "large target." Certainly he has provided historians with a challenging hypothesis that ought to stimulate discussion of this important issue.

RUDOLPH J. VECOLI
University of Minnesota,
Minneapolis

JAMES B. MURPHY. *L. Q. C. Lamar: Pragmatic Patriot*. (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1973. Pp. 294. \$11.95.

We now have a second major biography of the distinguished nineteenth-century politician, Lucius Q. C. Lamar of Mississippi, one of the few secessionist leaders to become prominent in national political life after the Civil War.

Murphy's recent work immediately invites comparison to W. A. Cate's study, *Lucius Q. C. Lamar: Secession and Reunion* (1935).

Why another biography? While Murphy never formally justifies his effort, it is clear from the text that this is revisionist history, less in terms of new facts or sources than interpretation. The current work does indeed, and perhaps properly, attempt to deflate Cate's overly eulogistic approach to Lamar. Nevertheless, this new interpretation—aside from some useful reassessment of Lamar's role in the Compromise of 1877—suffers from a basic lack of evidence. Murphy's characterization of Lamar as a politician without principle, who flew with the wind, just does not fit. Of course Lamar was often pragmatic—any politician must be—but his career also demonstrated great political courage on more than one occasion, enough that he was included in the late President Kennedy's *Profiles in Courage*. As for virtue and honesty, no one, including Murphy, has successfully accused him of a lack of either in a day when both characteristics were in extremely short supply. If Murphy is correct that Lamar's image was somehow contrived (pp. 272–73), he succeeded in accomplishing something almost no other nineteenth-century politician was able to do.

The most distressing difficulty with *Pragmatic Patriot*, however, is that at no point does Murphy's Lamar "come alive." The man whom Henry Adams once described (*Education*, p. 185) as an extraordinary personality "quite unusual in social charm" is faceless here. And Cate's extensive documentation of Lamar's great skill as an orator and raconteur is almost completely missing.

Unquestionably Lucius Q. C. Lamar had style. Cate succeeded in portraying him in terms that Henry Adams would have recognized and in a style of writing equal to that of the subject. The present biography, by contrast, is terribly dehydrated (it is half the length of the earlier work). It has the accuracy and swift sequence of a chronology, but also most of its defects as history. The plot is there, but the story is gone, and along with it, I am sad to say, most of Henry Adams's (and Cate's) Lamar.

PAUL P. VAN RIPER
Texas A&M University

JOSEPH LOGSDON. *Horace White, Nineteenth Century Liberal*. (Contributions in American History, number 10.) Westport, Conn.: Greenwood Publishing Corporation. 1971. Pp. xiii, 418. \$13.50.

Those who judge the merits of a biography by their desire to spend an evening in the company of its subject had best stay away from this one. Horace White, editor of the *Chicago Tribune* during the 1860s and 1870s and of the *New York Evening Post* during the 1880s and 1890s was, according to Henry Watterson, "an iceberg," and White himself stares out from the frontispiece to this volume looking not so much stiff as stuffed. It is thus a measure of Professor Logsdon's commitment to the craft of history that he has provided us with this detailed and enormously suggestive study of White as a figure symptomatic of nineteenth-century political and ideological dilemmas.

Although Logsdon's brilliant discussion of High Calvinist culture in the upper Midwest during White's boyhood provides clues to his personality, the book's focus is on his career. By anyone's standards Horace White was "important." As a young journalist in Chicago, he became active in the Illinois Republican party and in the antislavery National Kansas Committee. When war broke out White went to Washington to report on politics as a *Tribune* correspondent and then as a free lance. In 1865 he returned to Chicago to buy up John Locke Scripps's shares in the *Tribune*, of which he soon became editor-in-chief. In this capacity during the next ten years, he brought the paper national prominence and considerable political power through his advocacy of the Radical position on Reconstruction and equal rights.

In a Republican era, White rode the tide of Republicanism to wealth and influence. When he saw that tide ebbing, he looked for a new wave in the organization of the Liberal party of 1872. He guessed wrong. Defeated both in the Cincinnati convention, where he was a Trumbull man, and then more severely in the general election, White, and by extension the *Tribune*, lost not only prestige but also the close ties with the Illinois Republican party upon which the success of the paper depended. Where the defeat was personal for White, the *Tribune's* loss of status as a party organ threatened the paper's financial viability and

ultimately led to White's removal as editor in 1874.

In a well-ordered world White's career would have ended there. But then this would have been a less interesting book, a careful and well-documented but extremely limited study of the inside of politics during the mid-century. Instead, White went about picking up the pieces. With cash obtained from the sale of his shares in the *Tribune*, he joined Henry Villard in a variety of business enterprises, including purchase of the New York *Evening Post* and the *Nation*. All of these turned out to be financially profitable for White, and his return to journalism as an associate of Carl Schurz, E. L. Godkin, and Wendell Phillips Garrison restored him to a position of some prominence within the Republican party. As a spokesman for the neo-Whiggery of this group of Mugwumps and free-traders he could never achieve the influence he had possessed when he rode the Radical tide in the 1860s. Now he rode the wave of the past. Mistrustful of democracy and its consequences, like others of his generation he found himself an outsider in American politics, yet unwilling to pay the price to become an insider. Instead he turned to the study of political economy as a way of understanding his alienation and discovered structural defects in the American system at all levels. Real politics he could not understand, however, and, again like others of his generation, he reacted with equal hysteria to the emergence of class conflict and the exposure of corruption in government, to imperialist ventures abroad and high-tariff legislation at home, as if these were of equal moment.

It is the emergence of White's conservatism after the mid-1870s that most intrigues Logsdon, as an example of the "crisis of Liberalism" in the nineteenth century. Unfortunately, as an ideologue White characteristically made public problems into private issues, rather than the reverse, and his career can best be understood as a sometimes successful and sometimes unsuccessful attempt to follow the main chance.

HENRY D. SHAPIRO

University of Cincinnati

JULIUS WEINBERG. *Edward Alsworth Ross and the Sociology of Progressivism*. Madison: State Historical Society of Wisconsin. 1972. Pp. xii, 260. \$8.95.

A product of the middle border and a conspicuous member of the first generation of academic social scientists in the United States, Edward A. Ross (1866–1951) was the most popular sociologist of the Progressive era. He was, as Harry Elmer Barnes once said, "a crusading sociological publicist for a whole generation." The author of this trenchant, well-written biography does not portray Ross as a paradigm of the Populist or Progressive "mind," nor does he present him as a typical example of the muckraker or the prohibitionist, the reformer of the law, the nativist, or the rising, turn-of-the-century academician. Ross played each of these roles, but he managed to transcend all of them. Weinberg interprets him as a transitional figure in modern American intellectual history, a critic who sought "to bring into harmony the old and the new, to reconcile the values of the nineteenth-century, rural, Anglo-Saxon, Protestant countryside with the requirements of a twentieth-century, urban-industrial, polyglot nation" (p. xii).

Weinberg's study is based on a large collection of Ross papers and on the sociologist's voluminous publications. The result is an authoritative account of Ross's intellectual development and academic career, a terse, judicious evaluation of his scholarly writings, and an illuminating treatment of his place in American reform. Weinberg, while showing the limitations of Ross's sociological thought, makes a convincing case for his contributions as a creative scholar. In *Social Control: A Survey of the Foundations of Order* (1901) and other significant works Ross emphasized the need for an organic view of society and a positivistic methodology, he produced a pluralistic theory of social forces that seemed to reflect the realities of American life, and he stressed a concept of social processes that struck a balance between group determinism and individual freedom. Students of the Progressive period will find the chapters on "The Sociologist as Progressive" and "The Sociologist as Nativist" particularly instructive, though some readers may share my disappointment that a more thoroughgoing synthesis of the various elements in Ross's progressive thought was not attempted.

DEWEY W. GRANTHAM

Vanderbilt University

GEORGE E. MOWRY. *Another Look at the Twentieth-Century South*. Baton Rouge: Louisiana State University Press. 1973. Pp. ix, 90. \$4.95.

GEORGE BROWN TINDALL. *The Disruption of the Solid South*. (Mercer University Lamar Memorial Lectures, number 14.) Athens: University of Georgia Press. 1972. Pp. xiv, 98. \$4.00.

These two slender volumes of lectures update, if they do not materially revise, a chapter in more recent Southern political history. In the Louisiana State University Fleming Lectures Professor Mowry examines Southern political history against a comparable agrarian background of the western Middle Western states. He does this by constantly tapping the underlying vein of conservatism in the two regions. The Midwestern section after 1900 he says bore remarkable similarity to the South in its one-party loyalty, and the results flowing therefrom were approximately the same. Much of the substance of this provocative, brief study is taken up with comparisons of political phenomena that have controlled the mind and reactions to central issues in the two regions.

Mowry, like his colleague George Brown Tindall, concerns himself with the disruptive forces at work in the country that have tended to shatter Southern economic and political traditions. He takes a longer perspective on these forces than does Tindall. In his lecture dealing with "The Paradox of Southern Conservatism" Mowry traces the course of change through the political behavior of John Sharp Williams, Josiah Bailey, and James F. Byrnes. Mowry concludes that perhaps the party label meant little if anything to these three in the face of their determination to use it as an instrument to "maintain the existing socioeconomic society at home in the South, and, of course, to secure their own personal careers."

Mowry's final lecture, "The Persistent Elite," examines the anatomy of Southern conservatism in a high degree of reality. It was the issues rather than the romance that governed the course of Southern conservatism. Of these there were threats, antilynching legislation, taxes, wage scales, internationalism, race, and agriculture. Mowry's final lecture constitutes a rather full but succinct analysis of the course of action and reaction in the South during the past half century. The Southern

conservative elite exercised a remarkable control both regionally and nationally.

George Brown Tindall has dealt with the rise of the Republican party in the South largely since the inception of the Eisenhower ground swell in 1952. He has, obviously, emphasized the campaigns of Eisenhower, Goldwater, and Nixon as their Southern strategy approach had a bearing upon a region already involved in an enormous upheaval of social and economic change. Goldwater, he says, performed a role in the South similar to that of Al Smith in the Northeast in 1928. He helped to consolidate urban labor and ethnic groups.

Tindall gives a penetrating analysis of the efforts to wean the South over to the Republican party. He has described in some detail the "Southern strategy" as it has worked from within the region. He parades some of the master builders who have sought to effect a Southern defection from political traditions. But whatever prognostications about the degree and direction of change that has already occurred or will take place, one question still remains. What effect will the end of the George C. Wallace hold on a significant segment of voters have on the two major parties? Tindall views the change that has come within the sectional political organization during the past two decades as beyond concept in 1940. For the first time, the region has approached a nationally recognizable two-party system that gives some promise of durability.

These brief studies signal an end to traditional Southern social and political history. The South has felt the impact of enormous economic and social forces. Too, the impact of worldwide opinion has at least made the region a self-conscious one. Both sets of lectures are penetrating in their scholarship and provocative in their conclusions.

THOMAS D. CLARK

Eastern Kentucky University

HARVEY A. LEVENSTEIN. *Labor Organizations in the United States and Mexico: A History of Their Relations*. (Contributions in American History, number 13.) [Westport, Conn.: Greenwood Publishing Company. 1971. Pp. x, 258. \$11.00.

This is a well-written survey of seven decades of U.S.-Mexican labor relations. Professor

Levenstein demonstrates that during most of this century labor leaders on both sides of the border have favored contacts for a variety of personal, ideological, and practical reasons. Some of the factors were Sam Gompers's vanity and a desire to seal off the United States from revolutionary unionism, American labor's desire to halt strikebreakers and immigrants at the border, Mexican labor's hope of eliminating discrimination against Mexican workers in the United States, and the desire of both movements to be involved in political decisions in their respective countries.

The book's coverage is best, or perhaps more meaningful, during the years of Wilson's presidency. The period was marked by Sam Gompers's support for the Mexican Revolution and his creation of the Pan-American Federation of Labor. The account of later years treads more familiar ground and also seems less well documented. Between the time this work was finished (as his dissertation in 1966) and the time of publication, Professor Levenstein was a bit reluctant to make revisions. There is nothing, for instance, about such CTM actions as its 1966 rallies in support of the farm workers strike in the Rio Grande Valley of Texas. Nor is there any mention of the influx to the valley of green-card workers who scabbed against Mexican-American picketers while claiming that there was obviously no strike since, after all, workers were crossing the picket lines (an extremely rare occurrence in Mexico). Moreover, the author does not cite Ronald Radosh's *American Labor and United States Foreign Policy*; a dissertation concluded at the same time and place as his own, Henry Berger's "Union Diplomacy: American Labor's Foreign Policy in Latin America, 1932-1955"; or any of the dissident labor press in the U.S. and Mexico.

Though the reasons for contacts between the two labor movements have doubtless been real enough hardly a single issue has even been resolved as a result of these contacts. Indeed labor relations across the border have meant so little that allegedly important meetings have been postponed for years because of the cost of travel, and some leaders have regarded the expense-paid trips as a primary benefit of the boring conferences. Finally, after the author notes that the vast majority of union members

on both sides of the border played no role at all in the international proceedings and that most of the problems between Mexican and U.S. labor can be handled best at the state and local level, one is almost left to wonder whether this subject deserved an entire book.

GEORGE N. GREEN
University of Texas,
Arlington

DONALD SMYTHE. *Guerrilla Warrior: The Early Life of John J. Pershing*. New York: Charles Scribner's Sons. 1973. Pp. ix, 370. \$10.95.

That Father Smythe's research efforts, spread over a twelve-year period, were not misplaced becomes obvious with the most cursory inspection of his notes. Though his subject was no longer living when his quest began, many survived who knew the general during his early days. The result is the existence of valuable data that would be unobtainable now because of the demise of many of his sources. It is hoped that this material will at some future time be made available to other historians.

Smythe's Pershing differs significantly from the stern martinet of popular conception. While not neglecting his subject's driving ambition, the author fleshes out his character into a three-dimensional human being. Pershing could relax, fall in love, and grieve deeply at the loss of most of his family in a tragic fire. A high point is Smythe's discussion of Pershing's courtship and marriage. Pershing's facility in working with people of various races is examined in depth. Any study of Pershing's early life should deal with several difficult questions. Smythe handles the following in a convincing manner: Did Pershing live openly with a mistress in the Philippines and father several illegitimate children? What role did his father-in-law, Senator Francis E. Warren, have in Captain Pershing's being promoted over 862 senior officers to brigadier general? And, what responsibility did Pershing bear for killing women and children during the attack on Bud Bagsak in the Philippines in 1913? One early relationship, that between Lieutenant Pershing and Assistant Secretary of War George Meiklejohn, needs clarification. Smythe mentions that Pershing helped his friend Meiklejohn become assistant secretary, but the author does not

explain how the appointment was secured. The significance of such a close relationship between a junior officer and a high-ranking civilian official is not explored. Pershing corresponded directly with Meiklejohn throughout the Cuban campaign: one wonders what the effect of his frequent letters was on Meiklejohn. As Smythe indicates, Pershing's favor was repaid following his return to the United States when Meiklejohn saw that he was made chief of the War Department's Division of Customs and Insular Affairs, a post that Pershing's friends thought could lead to promotion to brigadier general.

Smythe's photos add much to the book. More sharply drawn maps would have been easier to follow. A map of Jolo was sorely missed. These items distract but little from a fine book. I eagerly await its sequel.

THOMAS R. STONE

United States Military Academy

REYNOLD M. WIK. *Henry Ford and Grass-Roots America*. Ann Arbor: University of Michigan Press. 1972. Pp. viii, 266. Cloth \$10.00, paper \$3.95.

This book sets out to document the impact of Ford's products and person on the mode of living and thinking of rural Americans. It begins by describing how the 15 million Model T Fords produced between 1908 and 1927 brought mobility to the population and a fundamental improvement in rural life. Later chapters cover Ford's tractors and agriculture and his views and interventions in such areas as science, economics, politics and education.

The rural masses, who considered him one of them—a champion of the little man—bombed Ford with personal letters. It is through innumerable quotes from these letters and newspapers of the era that the author illustrates the following that Ford attracted as a folk hero up until about 1929. The picture that emerges of Ford's importance in the minds of the rural population is impressive, as is the detailed research carried out by the author to build up that picture.

In the main the wealth of material has been well synthesized, and the book makes easy and interesting reading. Occasionally the numerous quotes used to make a point become something

of a litany—almost as if the author were reluctant to discard any of the pearls uncovered by his laborious research—but this is a minor criticism of a book that gives such a valuable insight into the Ford legend and its effect on rural society.

The book is also objective in its evaluation of Henry Ford; while fully recognizing his farsightedness in many spheres, it also shows his inability to match his thinking to the changing world. Thus we see him in the years before the depression as a progressive champion of the worker and farmer, while during the depression he appears as a man at a loss to grasp events, antagonizing his erstwhile supporters with crass pronouncements. (The depression was "only a state of mind that would pass when people became more flexible" was one of his comments.) Finally, we see him totally unable to come to grips with the New Deal philosophy.

And so Henry Ford the progressive, through stagnation of thinking, had become reactionary. The author could, with advantage, have illustrated this detachment from the interests of the little man with a description of the despotic manner in which he allowed his plants to be run in the last years of his life, a sad change from the man who was the first to pay his workers five dollars a day. Overall, however, this is an excellent book, worthy of a place on the bookshelf of anyone interested in history in the widest sense of the field.

COLIN FRASER

Rome, Italy

DONALD F. ANDERSON. *William Howard Taft: A Conservative's Conception of the Presidency*. Ithaca: Cornell University Press. 1973. Pp. ix, 355. \$15.00.

Professor Anderson, a political scientist, has produced a book that should go far to reduce the prevailing ambivalence in our estimation of William Howard Taft as president. Historians, and especially textbook writers, have tended to give Taft a low rating and then to offer excuses for his mediocre record. Drawing extensively on the voluminous Taft manuscripts and also examining the observations of such contemporaries as Archie Butt, Anderson unfolds a record that is devastating.

Anderson considers Taft's bad relations with

the public and the press; and for this he blames Taft himself. "Taft's indifference toward his public image," Anderson believes, "was the prime cause of his failure as president." Nor does Anderson score Taft high as an administrator, pointing out that he delegated too much authority, neglected to keep himself informed adequately on administrative affairs, was too hesitant to replace undesirable officials, and failed to inspire that "camaraderie and loyalty to the chief executive which had marked the Roosevelt administration." His administrative style gradually transformed the departments into nine independent kingdoms and created political difficulties.

The book contains interesting and meaningful comparisons between the views and performance of Theodore Roosevelt and Taft. On the much discussed matter of the degree of constitutional restraints on a president, the views of the two men were much more alike than their own declarations have led us to believe. After presenting convincing evidence to that effect, Anderson asserts that Taft's "constitutional views permitted him to be as big and strong as a Roosevelt or a Lincoln, if and when he chose to be."

Another similarity lay in the relationship of the two presidents with the Old Guard leaders in Congress. Both men found it necessary, or at least they concluded it was necessary, to cooperate rather extensively with leaders in Congress, such men as Nelson W. Aldrich and Uncle Joe Cannon. "The major differences, however, between the two presidents lay, finally, not in their cooperation with the established leaders, but in the degree of their public identification with them." Anderson's treatment of the nature and the impact of the exaggerated rhetoric of both Roosevelt and Taft is especially important to an understanding of how political styles of presidents can have an enormous effect on the nation.

The author does not examine every important aspect of the Taft presidency, wisely confining his effort to an in-depth treatment of important matters in which as a political scientist he has special competence. The result is a very important contribution to the gradual unfolding of a better and much needed understanding of Taft and his presidency. The

book is so timely, so sound, so fair, so well written, that anyone interested in political history, and especially of the presidency, will find it rewarding reading. One can read the book and in the end retain a good feeling toward Taft because of his decency, intelligence, and likability, and at the same time feel sorry that he occupied the White House.

HORACE SAMUEL MERRILL
*University of Maryland,
College Park*

SALLY M. MILLER. *Victor Berger and the Promise of Constructive Socialism, 1910-1920*. (Contributions in American History, number 24.) Westport, Conn.: Greenwood Press. 1973. Pp. xii, 275. \$11.50.

Professor Miller has added significantly to the limited body of published materials about Victor Berger. This volume is a meticulously detailed and well-documented account of his role in the leadership of the right or "reformist" wing of the Socialist party in its most significant years and in its subsequent decline.

The author presents Victor Berger as an embodiment of Max Weber's thesis that "moral man can participate in immoral society and yet retain his soul." In so doing, Professor Miller takes issue with Daniel Bell's thesis that the failure of the Socialist party in America was its inability to resolve this basic dilemma of ethics and politics, that it was "in but not of the world." She argues that Berger's record renders such an explanation of Socialist failure inadequate. Professor Miller is persuasive in arguing that Berger and his followers satisfactorily resolved the conflict between party ideals on the one hand and American political exigencies on the other. Had it not been for his personal characteristics of egotism, bossism, and aggressiveness, the author argues, Berger might have held undisputed leadership of the party and thus been able to fit it to American conditions. That they did not succeed is an important aspect of party decline.

The study is limited to an explanation of the differences within the party and within the reformist wing of the party during the years 1910-20 and thus becomes a valuable supplement to Ira Kipnis's detailed history of the party from its origin to 1913 with an emphasis on factional disputes.

The author disagrees with historians David Shannon, Kipnis, and Daniel Bell that the loss of the syndicalists in 1912 triggered the slow downward drift of party fortunes. The ramifications of World War I were the most likely factors contributing to the decline of the party. Contrary to Professor Miller's main thesis, it was on the war issue that Victor Berger betrayed the Weberian view and thus condemned the party to diminution. The party, with Berger's concurrence, chose to oppose the war, thus signaling the failure of his efforts to fit the party to American conditions. "Thereafter," the author maintains, "the revolutionaries refused to relinquish the initiative and Berger witnessed the disintegration of the party he had known."

The author has made use of all the major archives, and the result is a scholarly and substantial contribution to the history of the Socialist party in America.

D. JOY HUMES
Wells College

JOEL ARTHUR TARR. *A Study in Boss Politics: William Lorimer of Chicago*. Urbana: University of Illinois Press. 1971. Pp. xi, 376. \$12.50.

RICHARD J. CONNORS. *A Cycle of Power: The Career of Jersey City Mayor Frank Hague*. Metuchen, N.J.: Scarecrow Press. 1971. Pp. iii, 5-226. \$6.50.

Bossism has become a fertile historical field in the last decade. The literature of the field tends to cluster at the ends of a sloppy continuum. On one side, most work begins with the characteristics of populations and treats the dance of politics as an expressive act, displaying the potentials of public value agreement and conflict. On the other side, a few books and articles treat government as an industry producing goods and services, distributing them in varied markets under conditions of uncertainty, and inducing the risky investment of scarce resources.

Joel Tarr's study of William Lorimer is an example of the first approach. Lorimer was a turn-of-the-century Chicago politician who in 1912 achieved the distinction of being expelled from the U.S. Senate. The last third of the book, which deals with his abbreviated Senate career, has the flavor of reading the newspapers

for yesterday's scandals. Today's scandals are, however, a difficult act to follow, and I find little delight in old news. The first sections, however, are fascinating. Lorimer was a Republican operating within an ethnocultural setting that encouraged Democratic voting. Struggling with this electoral dilemma by softening the force of polarized political orientations, Lorimer (and countless politicians like him) created a middle ground in which governance was possible.

The form, dynamics, and output of the processes of governance are, however, not easily derived from Tarr's work. The omission is, in part, a measure of the topic. Lorimer, for all the angry charges against him, was not much of a boss. No man held Chicago in his pocket, certainly not William Lorimer. His hold over political resources was fragile; his options limited. Beyond this limitation of topic, the omission stems from an approach and purpose that are embedded in the concentration upon the expressive ethnocultural bases of politics. In the attempt to explain the incidence of electoral change and the symbolic uses of politics, the processes and outcomes of government are ignored, more by default, I suspect, than by conviction.

Richard J. Connor's study of Frank Hague is closer to the other end of the continuum and remedies the gaps in the ethnocultural literature. Connor begins, like Tarr, with an argument that Hague's power in Jersey City was rooted in the style and interests of second-generation Irish Americans. He quickly moves, however, to describe Hague's organization as an integrated bureaucracy conceiving of the population, the institutional structure, and the economy of the city as political resources and converting these resources into desired outputs: streets, schools, hospitals, and strengthened popular commitment. The decay of this commitment in the late 1940s was connected both to rigidities in the strategies of the organization and shifts in the resource base. It is difficult to know which was more important in Hague's defeat, his inattention and constricted recruitment or his failure to keep the streets clean and to counter the declining tax base of the city.

Despite its unpretentious format, Connor's book is one of the most interesting and im-

aginative in the entire bossism literature. It serves particularly well that old-fashioned but still valuable function of history to educate decision makers. It explicates constructive principles that novices can understand and may test both vicariously and in practice. (In contrast, the ethnocultural literature tells them little more than to watch out for "trivial" issues that are highly charged.) The major limitation of these principles is that they are drawn wholly from Hague's policy agenda. In at least one perspective, drawn from a larger concern with urban dynamics, the transition from the Hague to the Kenney machine was trivial. The interesting phenomena in Jersey City was its inability to influence the regional redistribution of population, income, and amenities despite enormous stability in its political leadership. Hague had a generalized and overt commitment to active, "progressive" government, and he was capable of sustaining coordinated policies over long periods of time in a manner rare in the United States. Yet he could not prevent—indeed he could hardly seriously address—the shabbiness that became Jersey City. I would be happier to pass on Connor's principles to my students if he had more fully explicated the constraints Hague accepted, the battles he did not fight and the powers he respected.

SEYMOUR J. MANDELBAUM
University of Pennsylvania

CLARKE A. CHAMBERS. *Paul U. Kellogg and the Survey: Voices for Social Welfare and Social Justice*. Minneapolis: University of Minnesota Press. 1971. Pp. xii, 283. \$10.00.

Clarke Chambers, author of *Seedtime of Reform* and director of the Social Welfare History Center at the University of Minnesota, has put his expertise to good use in this intellectual profile of the *Survey* and its editor Paul U. Kellogg. Although Kellogg began his career when he joined *Charities Review* in 1901, it was as editor in chief from 1912 to 1952 of the successor journal, the *Survey*, that he made his major contribution to social reform. Between his work for the two magazines, Kellogg coordinated the famous Pittsburgh Survey of 1908, the first important effort to analyze in depth the entire life of a single community by team research. From the Pittsburgh experience, Kellogg's

magazine took its name in 1909 at a time when the social-welfare movement was turning from charity to social action. After 1923 *Survey* was published in two autonomous, though allied, magazines—*Survey Graphic*, to educate the general public, and *Survey Midmonthly*, to keep the new profession of social workers abreast of their field.

Although Kellogg was disillusioned in his hopes of a thoroughgoing reconstruction of American society after World War I, the *Survey* magazines achieved their greatest influence and sustained circulation—30,000 for the *Graphic*—from the 1920s through the early New Deal years. Still, it was never easy going. Contributions from friends and foundations plus the practical skills of Paul's brother Arthur, who served as managing editor until his death in 1934, were necessary to keep the magazines alive. In general, *Survey* maintained a nonpartisan political stance, and Kellogg's initial support of the New Deal was modified by his disappointment over its failure to adopt a more comprehensive Social Security program. From 1935 to the end of the decade the magazines became more lively and independent, but divisions among the staff in regard to the approaching war and Kellogg's own increasingly heavy work load took their toll. After World War II financial problems proved insurmountable. In May 1952, six years before Kellogg's death, and over his stubborn protests, *Survey* published its last issue.

As Chambers points out, *Survey's* dual role, combining professionalism and popular reform, was being assumed by the technical journals and magazines of opinion. *Survey* thus became old-fashioned and out of touch with the post-war generation. By making full use of the extensive papers deposited at the University of Minnesota, Chambers succeeds in bringing to life both the *Survey* and its long-time editor.

ARTHUR A. EKIRCH, JR.
State University of New York,
Albany

DONALD E. GREEN. *Land of the Underground Rain: Irrigation on the Texas High Plains, 1910-1970*. Austin: University of Texas Press. 1973. Pp. xvii, 295. \$9.50.

Although specialized studies of particular land

company projects in the Texas panhandle region have appeared before, this volume is the first generalized treatment of the ground-water irrigation epoch in Texas high plains agriculture. It is an excellent study in every respect and brings to national attention the environmental preconditions and technological and entrepreneurial advances that started an agricultural boom in this most unpromising of geographical regions. At the turn of the century notice was taken of the deep-lying aquiferous stratum of Ogallala sand and gravel, varying from two to three hundred feet in thickness, underlying this region. Technological breakthroughs wedded the centrifugal pump and the internal combustion engine, using cheap oil, with sophisticated well-drilling techniques to bring up the precious fluid.

The first boom lasting from 1910 to 1917 was a land promoters' venture that demonstrated the efficacy of the pump in creating the new ground-water irrigation frontier. New conditions prevailed during the depression years when the combination of drought and low prices forced these high plains farmers to accept the new improved pumping technology in order to survive. Ingenious arrangements to pool credit, the introduction of more efficient and cheaper pumps, more powerful gasoline engines, along with experimentation with cotton, grain sorghums, winter wheat, and alfalfa, brought ground-water irrigation agriculture to a take-off point before World War II.

This scholarly depiction of the boom documents every aspect of unparalleled growth that kept farm youth on the land, augmented land values by gigantic strides, and introduced capital-intensive agriculture. The author is at his objective best in his somber portrayal of the decline of this form of agriculture occasioned by a rapidly falling water table. Apparently high-plains Texans are not unlike most Texans who, according to Justice William O. Douglas, adopt conservation measures that are too little and too late. Donald Green concludes that experience under the so-called water conservation statute of 1949 demonstrates that these farmers would rather continue to deplete their nonrenewable underground reservoir at today's scale of pumping than impair their vested rights in capital plant and water rights. All this absorbing monograph requires to make

the story complete is comparative data describing other examples of ground-water irrigation in the nation.

LAWRENCE B. LEE
California State University,
San Jose

E. CLIFFORD NELSON. *Lutheranism in North America, 1914-1970*. Foreword by KENT S. KNUTSON. Minneapolis: Augsburg Publishing House. 1972. Pp. xvi, 315. \$7.50.

Reading this book is like eating lunch at a Howard Johnson's. Certain kinds of historians' appetites are satisfied while others are left wanting. If one is looking for surveys, outlines, and charts to assist him in understanding the denominational differences, structures, and programs within Lutheranism, then this book will help. More particularly, if one wants to understand the broad outlines of the success and failure of the efforts of Lutheran denominations to unify themselves, then this work, written by a Lutheran professor of religion at St. Olaf's College, a committed ecumenicist, will be gratifying. In clear and lucid writing, Professor Nelson has described the "painful" and "agonizing" efforts of Lutheran denominations to iron out their differences in fellowship rather than demanding consensus prior to such union (p. 167ff).

From a methodological and conceptual standpoint, however, this work is more of the same standard fare that has dominated American denominational histories. With one notable exception this work is an extension of the William W. Sweet "school" of American religious history: Lutheranism is described in terms of its leadership; awkward theological controversies are mentioned but not examined or explained; and the interplay of social, economic, political, and cultural forces that affected American Lutheranism are broached but left undeveloped. For example, one continuing theme of American Lutheranism, according to the author, is the assimilation and "Americanization" of Lutheran immigrant denominations. The author claims, however, that Lutherans were generally opposed to the "culture-religion" of middle America (p. 167ff). Employing H. Richard Niebuhr's phrase, it would appear that many Lutherans endorsed a "Christ against culture"

stance. I see here a contradiction that is difficult to reconcile. How can Lutherans be "Americanized" and, at the same time, resist America's "culture-religion"? If such a proposition is true Professor Nelson would do American historiography a great service by amplifying his explanation.

The one exception to the above criticisms is the author's usage of data collected among Lutherans in 1969-70. This "profile of Lutherans" was the result of a survey given to nearly five thousand Lutherans. It produced some seven million answers to questions about values, attitudes, and beliefs. The survey, known as "A Study of Generations," revealed a wide variety of opinions among Lutherans on topics such as social change, theological commitments, clergy-lay differences, moral stances, and denominational programs. What we would like to know, however, is whether Lutherans qua Lutherans differ from other members of American mainline denominations. At least Professor Nelson gives us a hint as to how American religious history might be more quantified.

The most useful portions of this book may be the many pages of footnotes. They point to the vast reservoir of documents extant in denominational archives, many of which Professor Nelson has combed. Perhaps now we can await from him a fuller explanation of their meaning. After all, lunching at Howard Johnson's does not necessarily preclude dinner at Henri Quatre.

JOHN W. STEWART
Hope College

ROBERT MOATS MILLER. *How Shall They Hear without a Preacher? The Life of Ernest Fremont Tittle*. Chapel Hill: University of North Carolina Press. 1971. Pp. xii, 524. \$12.50.

In a work that is both solid and smooth, Professor Miller continues, through Tittle, to examine twentieth-century American Protestantism, the better to understand American culture and society. He is thoroughly convincing.

Preeminently a preacher, Tittle filled for thirty-one years (1918-49) the prestige-laden pulpit of First Methodist Church, Evanston. Armed with physical vigor, a penetrating voice, an inquiring mind, broad reading habits, a bulldog tenacity, and a sympathetic wife, he con-

vinced his congregation's conservative lay leaders to understand, even if they could not always accept, his advanced Social-Gospel stands. Thus the official board reiterated their support of the ideal of a free pulpit. Throughout his "prophetic preaching," Tittle urged nonviolent resistance, economic reform (he regularly voted for Norman Thomas), racial justice, and ecumenical cooperation, as well as the usual Methodist tilt on temperance. Not content with mere advocacy, Tittle encouraged institutional church involvement in community issues and services. Moreover, he led in fostering the Christian life, through Sunday education, pastoral counseling, and esthetic enrichment based on Tittle's Ralph Adams Cram-inspired reconstruction of the sanctuary. So enthusiastic were his parishioners that one observer ruefully called them "a congregation of damned Tittleites."

Miller offers no "authorized" biography. Yet he builds on unrestricted access to family papers (except for one group of scissors-censored letters to the preacher's wife), the well-kept archives of First Church, and interviews with Tittle's widow, children, other family, and associates. To the resulting immersion in congregational life, Miller adds Parkman-like insights from his own recollections of the Evanston environs. He rarely misses a resource, although the Methodist *General Minutes* might expand the data about Tittle's early Ohio churches. Miller's organization—chronological a third of the way, then topical—both heightens analysis and insures repetitions. The volume's 200,000 words bring a leisurely pace that occasionally threatens surfeit. At least, Tittle frequently speaks for himself, as: "Granted that earth can never be heaven, it hardly follows we should allow it to become hell."

Altogether, the study is well done. Miller has given us a virtually unique, detailed view of a successful local-church pastor at work. What next, Bob? Harry Emerson Fosdick?

THEODORE L. AGNEW
Oklahoma State University

FRANCIS PICKENS MILLER. *Man from the Valley: Memoirs of a 20th-Century Virginian*. Chapel Hill: University of North Carolina Press. 1971. Pp. xvii, 253. \$8.75.

This is an interesting autobiography of an unusual humanistic liberal. From a long line

of Presbyterian clergymen he received an early religious evangelism. From the start of his educational career Francis Miller demonstrated an enduring affinity for causes of a compassionate humanistic nature.

After graduating from college with a Phi Beta Kappa key he joined the YMCA on the national level. His service there gave way to active military service during World War I. At the end of the war he entered Oxford University and subsequently became a Rhodes scholar. As a leader in the British Student Christian Movement he lectured on college campuses throughout the British Isles.

Back in the United States he became a leader in the American Student Christian Movement and later served as secretary of the World's Student Christian Federation with headquarters in Geneva, Switzerland. After serving in Geneva for three years he returned to become a lecturer at Yale. Miller states that his work with WSCF was excellent preparation for his leadership role in the World Council of Churches after World War II.

As field secretary of the Foreign Policy Association, he promoted its expansion throughout the country. When the war began in Europe he organized an American war spirit against Hitler. He joined the Office of Strategic Services. Stationed in London, his contributions were significant in the OSS and later in SHAEF.

With the Army of Occupation in Berlin, he secured valuable German documents that found their way into American archives. In Berlin he witnessed the debasing of the character of the American soldiers through black-market activities and moral decay. He returned to American civilian life as the gentleman from Fairfax.

The last part of this delightful memoir deals with Miller's political activities. As a member of the Virginia Assembly he refused to join the Harry Byrd reactionary machine. Defeated in 1949 by the Byrd-selected John Battle for governor, he lost to Byrd in a race for the United States Senate in 1952. His efforts to reunite the northern and southern Presbyterian churches failed also. He defeated the Byrd machine, however, on the integration issue in Virginia. Indeed, he frequently led movements before the masses were ready for the reforms. As a member of the State Department from 1961 to 1965, he liked Kennedy's innovations

but hated Johnson's "business as usual" approach.

Francis Miller has rendered a service to American liberalism and to the understanding of Virginia politics by publishing his memoirs.

GEORGE OSBORN

University of Florida

PAUL L. MURPHY. *The Meaning of Freedom of Speech: First Amendment Freedoms from Wilson to FDR*. (Contributions in American History, number 15.) Westport, Conn.: Greenwood Publishing Company. 1972. Pp. x, 401. \$14.50.

As with his *The Constitution in Crisis Times*, Murphy points to World War I and its immediate aftermath as commencing a new era for the freedom of speech in American society. This book is different, however, in that it approaches the meaning of free speech less through pronouncements of the high court than from an examination of the sociointellectual record. In this sense it is not constitutional history of the sort we normally expect. Neither should it be compared to the closely argued studies of William Finley Swindler and Zechariah Chafee. Rather, it is a selective chronicle of conflicting interpretations, chiefly during the years 1919-31.

As a narrative, it is richly documented and splendidly told. The ingredients from which attitudes of the time were fashioned are found in the "red scare" mentality of the immediate post-war years: the multiplication of sedition and criminal-syndicalism statutes at the state level; the coal and steel strikes of 1919; New York's legislative fight over the seating of properly elected socialists; and the use of blanket search warrants, raids, and deportations under Attorney General Palmer. From the welter of such events congealed the stiff-necked conservatism of "normalcy."

A good deal of Murphy's material deals with episodes in which the ACLU was involved. And during the 1920s these were primarily problems of labor. Much of this was the result of a suppressive alliance between industrial establishments and local law enforcement officials, as with the violence in Logan and Mingo Counties, West Virginia; beatings by the coal and iron police of Pennsylvania; or shootings at Columbine mine in Colorado. Fundamentalist controversies and academic freedom are men-

tioned but given far less attention than disputes with labor. There is, however, an abundance of information regarding societies like the DAR, the American Legion, and the Better America Federation. And Murphy skillfully traces the reappearing usage by such groups of influences such as New York's Lusk Committee findings or Mrs. Lucia Maxwell's "Spider Web Chart," which purported to demonstrate the interlocking leadership of dissident organizations throughout the United States.

The book finds the Hoover administration marking a liberal turn in attitudes toward dissent. A number of developments are adduced to support this conclusion but especially the Near and Stromberg decisions with their incorporation of federal restraints into the due process clause of the Fourteenth Amendment. If it is difficult to discern the "subtle" meliorism Murphy contends the twenties to have generated, especially in the face of so much oppression and a steadfast commitment to property interests by the Taft court, there are abundant and persuasive grounds for agreeing that the early thirties mark a definite departure from the brutalities of Passaic, Gastonia, and Marion or from the brooding surveillance of the likes of Ralph M. Easley and William J. Burns. And, the book does convincingly document the many divergent employments to which the notion of free speech was put, illustrating the fractured nature of a decade traditionally seen as something of a philistine, sociolegal unity.

Everything considered, as an account of the free-speech issue during the twenties, it is a solidly constructed and indispensable work.

B. CARMON HARDY
California State University,
Fullerton

HAYWOOD S. HANSELL, JR. *The Air Plan That Defeated Hitler*. Atlanta: [the author.] 1972. Pp. x, 311.

FLINT O. DUPRE. *Hap Arnold: Architect of American Air Power*. (Air Force Academy Series.) New York: Macmillan Company. 1972. Pp. xiv, 144. \$5.95.

In World War II airpower added new dimensions to warfare both as a part of combined operations and in its own right as an independ-

ent striking force. Tactical air support was requisite for all surface operations, while strategic air campaigns conducted by the United States and Great Britain undermined the will and capability of the Axis powers. Among American air commanders it is fair to say that strategic air operations were always thought to be more important than tactical air support, but, ironically, the historical treatment of tactical air operations has been much more extensive than that of the strategic air campaigns. Most military service histories inevitably cover air support to some necessary extent, but independent strategic air operations have been an intimate concern only in air histories. Where it might have been expected that the U.S. strategic air commanders of World War II would have provided enlightening memoirs, very few of them have elected to do so. In Great Britain, Sir Charles Webster and Noble Frankland have authored a splendid history of the Royal Air Force Bomber Command and British air commanders have been notable memorialists, but British strategic air experience was only half of the story of the Combined Anglo-American Bomber Offensive against Axis Europe. Moreover, British strategic air experience was significantly different from the kindred endeavor of the U.S. Army Air Forces. Through all these circumstances, the historical memory of U.S. strategic air operations in World War II has grown dim and has lost much in perspective in the quarter century since the defeat of Germany and Japan.

In the U.S. Army Air Corps of the 1930s, Captain Haywood S. Hansell, Jr. was already recognized as a coming thinker and planner, and he continued to distinguish himself in the wartime Army Air Forces as a staff planner and strategic air field commander. In the latter role, he commanded B-17s in the United Kingdom in 1942-43 and the XXI Bomber Command in B-29 attacks against Japan in 1944-45. He was recalled from retirement during the Korean War and finally retired in 1955 with the rank of major general, U.S. Air Force. In *The Air Plan That Defeated Hitler*, General Hansell has taken a major step toward telling the story of the U.S. strategic air offensive in Europe in its own right. His narrative recalls the development of U.S. strategic air thinking through the Air Corps Tactical School

in the 1930s, describes the engrossment of the concepts in major Army Air Forces planning prepared in Washington in 1941-42, records the application and modification of the planning that resulted from aerial combat over Germany, and finally appraises the effect of the strategic bombing from the standpoint of original conceptions. As a historical narrative Hansell's effort is important for the perspective that it provides; in addition, his personal reminiscences are significant vignettes of air force history. Some merit of his work is indicated by the evaluation of the American strategic air commander, General Carl A. Spaatz, who has said that this is the "most complete and authoritative book" on the subject that he has seen. *The Air Plan That Defeated Hitler* was privately published and was given a fairly wide distribution. In addition, it may be obtained on interlibrary loan from the Air University Library, Maxwell Air Force Base, Alabama.

In a brief description of General Henry H. Arnold, Hansell gives high praise to the wartime U.S. Army Air Forces commander who deserved the credit for the building of the air forces prescribed in planning. A more extended look at this remarkable air officer is provided by Flint O. DuPre in a highly appreciative biography entitled *Hap Arnold: Architect of American Air Power*. DuPre is a long-time and lately retired member of the Secretary of Air Force Office of Information and offers a brief and highly readable account of General Arnold's pioneer association with U.S. military aviation from his first flights in 1911 through his retirement in 1946. The volume will be of interest to younger readers, but—quite unlike Hansell's *Air Plan*—it offers little to a serious and critical historian.

ROBERT F. FUTRELL
Air University

GARY R. HESS. *America Encounters India, 1941-1947*. Baltimore: Johns Hopkins Press. 1971. Pp. xi, 211. \$9.50.

L. P. MATHUR. *Indian Revolutionary Movement in the United States of America*. New Delhi: S. Chand and Company; distrib. by South Asia Books, Columbia, Mo. 1970. Pp. vii, 169. \$7.00.

Any study of India's freedom movement would reveal that the Indian leaders expected under-

standing, sympathy, and even enthusiastic support for their cause from the American people and the American government. A section of the American people became interested in India's independence and some prominent liberals even championed the cause. By and large, however, the general public in the United States and more particularly the Roosevelt administration were naturally preoccupied with the war against the Axis powers. They were forced to respond to the nationalist struggle in India because of its potential for adversely affecting the war effort in the India-Burma-China theater. The record of the American response, unfortunately however, was not unequivocal and remained ambiguous at its best. The American government was confronted with the dilemma of choosing between the prized ally and the nationalist movement seeking independence from that ally. From the perspective of hindsight we can see that the United States was not really faced with such a clear cut either/or choice in the matter. The British needed the Americans much more than the other way around. By an astute combination of resolute opposition, emotional outbursts, and the sheer force of personality, Winston Churchill was able to brow beat Roosevelt into a retreat. He succeeded in making the U.S. government defer to the British on all important matters relating to "the Raj." Successive American governments traditionally considered India to be the British responsibility. Habits die hard.

Roosevelt undoubtedly had a fund of goodwill and sympathy for popular struggles for national self-determination in general and the Indian nationalist movement in particular. The available evidence, as demonstrated by Gary Hess and many other recent Indian and American writers, indicates that Roosevelt believed the British tactics of delay and repression to be wrong, unwise, and likely to be ineffective in any case. But, when forced to choose between the two (the British and the nationalists), he shied away from his better judgment and deferred to the wishes of Churchill, the adamant. Gary Hess delineates the contours of the ambiguous American response during the war and early postwar years with clarity and skill. Since 1945 the U.S. was confronted with similar choices elsewhere in Asia and Africa, and the American response to India's na-

tionalist movement, Hess rightly argues, constitutes an illuminating case study. The detailed analysis of America's encounter with India in the forties is preceded by a rapid, historical survey of the earlier developments in India and the American response to them.

One of the exciting episodes in this story was the so-called "Ghaddar" or the Indian revolutionary movement that flourished in the United States during the first two decades of this century. L. P. Mathur tells of the heroic but futile struggle of the revolutionary exiles who congregated on the West Coast and elsewhere in the United States and Canada. The American government kept a close watch over the activists. According to Mathur, however, they were allowed to carry on their propaganda activities, including plotting and planning against the British rule in India. The British government's protests in this regard were mostly ignored (except for a brief period after 1917) because such scheming unaccompanied by any overt action was not against the American law. But when the Indian revolutionaries started preaching anarchism, they became liable to legal action. Many of the leaders were arrested and deported to India for plotting against the British government in collaboration with some German agents. During this period the attitude of the American government was "liberal," on the whole, Mathur concludes. One only wishes that Mathur in his account of the "heroic" exploits of the revolutionary exiles and especially of the scope and reach of their activities in the United States were less prone to accept at face value the exaggerated versions recorded by the revolutionaries themselves.

The detailed analysis presented by Hess highlights a recurring pattern in the American response to key developments in India and the British policies toward them. The inside stories related by Hess regarding the Cripps plan, Johnson mission, Chiang's telegram to Roosevelt, the Merrill plan, the Phillips mission, and the like revealed the nature of the American response and the constraints within which it occurred. The U.S. government felt compelled to recognize the need for a new initiative to break the deadlock between the nationalists and the British; made sincere behind-the-scene efforts to put across to Whitehall some potentially fruitful avenues as suggestions from a

friend and ally; then came the inevitable Churchillian response—the resolute and inflexible stand, followed by the predictable and quiet retreat on the part of Roosevelt. The British propaganda in the United States was successful indeed, especially after the failure of the Cripps mission. From that time on the tide of popular sentiment in America turned in favor of the British government and against the Indian nationalists. Hess ably brings out Roosevelt's failure to comprehend the continuing importance of India and the impact of U.S. retreat on Indo-American relations. *America Encounters India* is a well-written and well-researched book and deserves wide attention.

B. RAMESH BABU

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CHIN-TUNG LIANG. *General Stilwell in China, 1942-1944: The Full Story*. (Asia in the Modern World Series, number 12.) [Jamaica, N.Y.: St. John's University Press under the auspices of the Center of Asian Studies.] 1972. Pp. xviii, 321. \$6.95.

As one of the most controversial figures in the history of Sino-American relations, General Joseph W. Stilwell has been the subject of many books. Since these works are written by Americans and based on American materials, they have been criticized by Nationalist officials and Chinese scholars as one-sided, representing only the American point of view. In the present work Chin-tung Liang of the Academia Sinica sets out to correct what seem to him unfounded American assumptions and misrepresentations.

After an extensive study of American and Chinese documentary materials, especially the unpublished private papers and official archives of Generalissimo Chiang Kai-shek, Liang came up with a lengthy bill of indictment against General Stilwell. The latter is described as an arrogant, vindictive, jealous character with an intensive disdain and hatred of Chiang Kai-shek, his supreme commander in the China theatre of war. Stilwell, on Chinese evidence, was guilty of "insubordination" five times in a short period of three months between 1943 and 1944. He went so far as to threaten moving the United States forces from China to Russia if Chiang did not appoint him commander of all Chinese forces.

Liang rejects the narrow approach that the

episode of Stilwell's recall can be explained by personality conflicts alone. The author traces the fundamental cause of the conflict to the strategic differences between the two nations. As soon as the island-hopping operations across the Pacific proved successful, the United States de-emphasized China as a military base for attacks on Japan. As strategies changed, China's needs were ignored. She was not represented at the Combined Chiefs of Staff Conference, which decided on strategy; lend-lease material assigned to her was under the control of Stilwell, while neither Britain nor the Soviet Union was subject to such control. As the author shows, this "unequal treatment" inevitably led to conflicts.

Liang considers it a gross injustice that China, cooperating with the United States to the best of her ability, should have been subject to pressures, threats, and abuses from the Americans. His indignant criticism departs from the impersonal manner of the objective historian, but the book is a forceful presentation of the Chinese case. If Liang gives little attention to American interests and difficulties, his arguments for China are firmly based. This provocative study deserves the attention of all serious students of Sino-American relations during World War II.

CHESTER C. TAN
New York University

WILLIAM M. BUELER. *U.S. China Policy and the Problem of Taiwan*. Boulder: Colorado Associated University Press. 1971. Pp. 143. \$5.95.

During his five years in Taiwan this Chinese-speaking author carefully inquired into the attitudes of Taiwanese and Mainland Chinese. He found the latter committed to but no longer believing in the Chiang Kai-shek-Dulles return-to-the-mainland doctrine. The Taiwanese were uniformly anti-Mainland Chinese and anti-Communist, desiring though not inclined to fight for self-determination for Taiwan. Here, in his chapter on "Political Realities of Taiwan," is Bueler's main contribution.

The bulk of his book, however, is devoted to reviewing and analyzing White House and State Department pronouncements on Taiwan from 1949 to 1970. The review is clear and reasonably well balanced. It shows how the

Korean War caused Truman and Acheson to embrace the Chinese Nationalists; how the embrace tightened under Eisenhower and Dulles, the main architects of America's policy toward Taiwan; how that policy held under Kennedy and Johnson and began to change only after Nixon became president.

The analysis emphasizes Washington's regrettable lack of interest in the Taiwanese independence movement; the persistence of the collapsing-communism-and-mainland-recovery myth as a factor in American policy; and the United States conviction that there is no give in the Communist position on Taiwan (wrong, says Bueler, Peking might "in time, reconcile itself to Taiwanese self-determination").

Like many who write on Sino-American relations, Bueler hopes to influence policy makers. One problem is that he wrote before Nixon's pilgrimage to China, so his advice is based on a situation that has changed. But, since the question of Taiwan remains unresolved, the volume is still worth attention for policy discussions as well as for the historical record.

Saying that, one must add a word of disappointment. The documentation is thin and, unfortunately, is confined almost entirely to American published materials. Even there, much is to be desired in newspaper and magazine coverage. Some pre-1949 material would have been welcome. And it is disturbing to find policy-making discussed with only the most incidental reference to the impact of economic, religious, political, and military forces, and without much consideration of other Asian and world developments.

FRED HARVEY HARRINGTON
University of Wisconsin

JOHN MORTON BLUM, edited and with an introduction by. *The Price of Vision: The Diary of Henry A. Wallace, 1942-1946*. Boston: Houghton Mifflin Company. 1973. Pp. x, 707. \$15.00.

Henry A. Wallace has often been pictured as mystical and incomprehensible. Having co-authored a biography of the man, I have never thought this to be true. Blum's wartime diary of Wallace reinforces my impression. Wallace's ruminations reflect the same pattern of ideas consistently expressed either in public speeches and writings or privately in correspondence.

They revolve around his idealistic and progressive political philosophy based on the twin themes of domestic abundance and international cooperation. Formulated while a rural editor, they were implemented as secretary of agriculture and expanded and amplified as vice president, secretary of commerce, and a presidential candidate. While the fundamentals of this philosophy remained the same it was the milieu that changed—and this made for controversy. As secretary of agriculture he compromised his ideal of the ever-normal granary with the interests of Southern and Midwestern conservatives (i.e., the so-called marriage of corn and cotton) to achieve a workable farm program. But as vice president and secretary of commerce, and the diary reveals this very clearly, he became less realistic and more doctrinaire in direct proportion to his lack of expertise in the field of foreign affairs. The more difficulties encountered and the less workaday knowledge he possessed, the more prone Wallace became to deal in abstractions. International affairs finally appeared to him only in the two-dimensional framework of one world versus atomic destruction with no other viable alternatives.

Mr. Blum has edited about 25 per cent of the total diary on the basis of what he considered a representative sample. He has included a twenty-four page biographical sketch written in beautiful prose and based on recent authoritative books. Helpful are numerous explanatory footnotes that identify personages mentioned in the text. No attempt was made, however, to correlate the diary entries with other manuscript material, except for the "Columbia Oral Memoir," and with this only occasionally. The only really new information revealed might be designated as political gossip. Here we get a candid appraisal of the duplicity and maneuverings of Franklin D. Roosevelt, the corruption of the big city bosses, descriptions of political infighting, the conniving of presidential assistants, and suggestions of possible illegal campaign contributions. Apparently political skulduggery was not invented with Watergate.

Reactions to the diary will depend upon how one interprets the cold war era. Some will see Wallace as a prophet whose prescience was

amazing, while others will find him a man capable of unlimited self-delusion.

FREDERICK H. SCHAPSMEIER
University of Wisconsin—
Oshkosh

Foreign Relations of the United States, 1947. Volume 3, The British Commonwealth; Europe. (Department of State Publication 8625.) Washington: Government Printing Office. 1972. Pp. xiv, 1131. \$5.75.

A short review can only begin to express the wealth of this volume, most of which recounts American diplomacy with Britain, France, and Italy and the genesis of the European Recovery Plan during the economic crisis of 1947.

It is fitting that the documents on Britain's trade and currency-exchange crisis appear first, since the solution of this problem was essential to America's European policy. As the British themselves put it, if Britain failed she would no longer be "the differential gear" between the American economy and those of other countries (p. 47). That American officials agreed was shown by their willingness to accommodate the British in interpreting the Anglo-American Financial Agreement of 1945 and by the extension of a further credit of \$400 million at the end of 1947.

In early 1947 American policy makers also expected the precarious economic and political situations of France and Italy to worsen and possibly collapse, leaving the way to power open for the French and Italian Communist parties and stimulating a return to economic autarky by all the European countries. From Paris and Rome the communist danger was portrayed in abundant and knowledgeable dispatches by Ambassadors Jefferson Caffery and James C. Dunn. One should note, however, that Caffery and Dunn also dreaded extremists of the right. Their preference—and that of their superiors—was for a democracy of the center, reflecting still the hopes for Europe that Americans had nourished during the recent struggle against fascism.

In Washington those who conceived the European Recovery Program were concerned about both communism and economic nationalism in Western Europe, so that some interpreters of these documents may well perceive a capitalist, open-door strategy. That, however,

would not be a full reading. These materials also illustrate an American belief in multilateralism as a community value and a genuine concern for the material health of Western civilization. Otherwise, the sense of drama that still pervades the memorandums and dispatches would be inexplicable.

That drama is further explained by the luminous State Department of 1947. Marshall's precise authority, Under Secretary Dean Acheson's rigor and tenacity, and the thoughtfulness of the first Policy-Planning Staff enliven these pages. The department's *esprit* is preserved in Division Chief Charles P. Kindleberger's good-humored recollection of who deserves credit for the Marshall Plan (pp. 241-47). His account of how the secretary of state came, finally, to accept a Harvard degree is both amusing and indicative of Marshall's quiet decency.

Altogether, the story of America, Britain, and Europe in the critical year 1947 is remarkable and many-sided, one which does not fit very well into the "origins of the cold war" mold. Hence it may be fortunate, as well as ironical, that as the State Department archives open, the latter controversy has entered into a somewhat pedantic phase. Perhaps the records of 1947 may now be studied from less sectarian and more historical perspectives.

ROBERT E. BUNSELMAYER
Yale University

Foreign Relations of the United States, 1948. Volume 7, The Far East: China. (Department of State Publication 8675). Washington: Government Printing Office. 1973. Pp. vi, 887. \$6.50 postpaid.

The most revealing aspect of this latest of the China volumes in the Foreign Relations series is its size: 887 pages, quite compact compared to the 2,963 pages for 1946 and the 1,477 pages for 1947 (see *AHR*, 79 [1974]: 249-50). Although its emissaries still struggled in the China tangle as the Red Hornet closed in, by 1948 Washington was shifting its attention back to Europe. Gone are the chapters and appendixes on loans, planes, ships, military equipment, taxes, property rights, transport agreements, and the countless other preoccupations of the White Man's Burden. What remains are Wagnerian final scenes with principals like

Ambassador Stuart, befuddled in Nanking, and Acting Secretary of State Lovett, exasperated in Washington. The supporting cast includes Consuls General Clubb (Peking) and Ward (Mukden) who stuck by their posts sturdily awaiting the People's Liberation Army.

The drama can be grasped neatly in miniature. Holed up in Mukden, Consul General Ward observed on December 31, 1947 that the situation in his bailiwick was "serious but not critical." When we last hear of him 848 pages later, on December 28, 1948, his colleague in Tientsin has said of Ward's headquarters: "Consulate General radio reported unharmed but guarded at all times by sentry. Sentries also guard office and residences." The sentries were Marshal Lin Piao's infantrymen.

Stuart must have driven Washington mad with dispatches, now piously upbeat, now sepulchral. On March 22, for no discernible reason, he claimed that "the friendliness [of the Chinese] towards our country and their trust in our intentions are almost universal and very deep. . . . We are helping this vast, amorphous population to adjust itself to modern conditions after the shattering of its ancient political and social patterns . . . (they) are in no sense a decadent race. . . . I have a sense of exhilarating expectancy despite all the questionable obstructions and uncertainties." By March 31 the situation was "desperate," and "either those of weak heart will prevail," or "those of stout heart will rally round a Generalissimo in some way to restore benevolence to his despotism. . . . We hope for [the] latter, but we fear [the] former."

Such swoops of mood, punctuated by tough advice to concentrate "our military and economic aid in all-out effort to assist the government . . . more bold and more imaginative measures are needed . . . rush shipment of arms and ammunition," etc., roused Acting Secretary Lovett to cable Stuart from Washington to knock it off. Lovett summarized a series of past Stuart communications that wittingly or not revealed KMT chaos, corruption, and desperation. Referring especially to the ambassador's recommendation to go "all-out," Lovett said: "In light foregoing appraisal, recommendations . . . seem inconsistent . . . and [fly] in face all previous experience US advisors in China."

Stuart's nutty moralizings and unreliability,

the somewhat stunned reportage from the northern consulates, and the bitter realism of Consul General Cabot in Shanghai ("The same old inefficient grafters are still occupying the same old positions") must be set in the wider context of Truman and Marshall who continued to enunciate a policy unrelated to circumstances. In his March 11 press conference, included in this volume, while the walls were falling around the ears of his legates in Manchuria, the president declared: "We did not want any Communists in the government of China or anywhere else if we could help it." On November 29, when the flight to Taiwan was already being hinted in dispatches, Marshall informed Stuart that the "Present National Government [is] the only instrument now available which has any capability of bringing about an independent stable China friendly towards the US. . . . A coalition government including the Communists would likewise be unsuitable as an instrument for achieving American objectives in China." (Later in this directive Marshall added "we have endeavored to impress upon the Chinese the advantage which we consider inherent in the American system of private enterprise.")

Along the edges of the main stage appear little comic turns: a Tibetan trade mission purveying yak tails shows up in Washington seeking an audience with President Truman. Cables flash back and forth between Nanking and Washington as the Chinese insist that Tibet, a mere particle of the Republic of China, "has no authority to conduct diplomatic negotiations with foreign governments." The puzzled Tibetans end up visiting with Secretary Marshall, leaving their white scarf and other momentos from Lhasa with Truman's chief of protocol.

Consul General Ward takes the new Communist mayor of Mukden on a tour of the USIS. The latter offers some "Commie" literature for the library. Ward, not in a good position to take a high hand, advises his chiefs that a "minority display" cannot hurt. Nanking and Washington say no. Shortly before the sentries arrive Ward cables back: "I feel that when formulating policy on USIS activity in China we should bear constantly in mind that with coming of [the] Communists the days of old China with foreign establishments enjoying

special privilege have in large measure passed."

JONATHAN MIRSKY
Dartmouth College

WALTER JOHNSON, editor. CAROL EVANS, assistant editor. *The Papers of Adlai E. Stevenson*. Volume 3, *Governor of Illinois, 1949-1953*. Boston: Little, Brown and Company. 1973. Pp. xiii, 621. \$15.00.

The third volume of *The Papers of Adlai E. Stevenson* covers his period as governor of Illinois. In the losing battle that fact fights with history, these four years of our hero's career—the only years in which he held elected office—are always going to have to struggle for recognition. The years in the gingerbread mansion in Springfield do not follow neatly on the New Deal and United Nations curtain-raisers, nor do they prepare us for the more than national impact of the 1952 campaign and the emergence of the world-resonant voice of American liberalism. There is a disposition to see them as a necessary interlude—a sort of sop to the democratic Cerberus before the born leader can enter upon his real role.

Volume 3 exposes the falsity of such a view. It is a carefully documented record of a phase that Stevenson himself regarded as genuinely important in itself and not merely as an apprenticeship or a probation. Although the letters contain a fair amount of grunting at the fardels to be borne, they are also quite explicit about the enjoyment in the fight and the pleasure in the achievements that the governorship alone makes possible. Although he would not relax his own style and standards—"I simply can't use the conventional banalities of politicians, even if I should" (p. 306)—he did not duck any of the grind—"one day last week I spoke eight times between 10 a.m. and midnight in Chicago and suburbs" (p. 314). The real meat of the volume is the day-to-day stuff of state government: patronage problems, gasoline taxes, law enforcement, handshaking, and budget-paring. The sauce to this rather tough steak is provided by Stevenson's own infinite capacity for endowing every encounter with a touch of his own grace and humor. These *Papers*, like all such collections, carry their heavy quota of obligatory "bread and butter" and courtesy letters, but there can be few official correspondents who have imparted a comparable degree of felicity

and gaiety to this small change of public life.

Yet much of the gaiety is gloss. The subplot to this volume is the story of Stevenson's broken marriage, and the dateline for many of these items is "my lonely room in the deserted Mansion" (p. 258). Stevenson the man competes for interest in these pages with Stevenson the governor.

As in volumes 1 and 2 the quality of the editing is as high as could be wished, accurate in detail, well judged and informative—what, in another context in this volume, the governor himself endearingly describes as a *succes faux* (p. 418).

H. G. NICHOLAS
New College,
Oxford

JAMES T. PATTERSON. *Mr. Republican: A Biography of Robert A. Taft*. Boston: Houghton Mifflin Company. 1972. Pp. xvi, 749. \$12.50.

Americans have traditionally undervalued, even demeaned, political skills in their chosen leaders, revering most those individuals in public life who have seemed somehow above or apart from the world of partisan politics. Yet in the final analysis the stability of the American system has rested on the services of politically aware and adept officials who have excelled at their jobs. Such a man was Robert A. Taft, who rose to such prominence in his party and in the United States Senate that he earned the sobriquets "Mr. Republican" and "Mr. Congress," respectively.

James T. Patterson portrays Taft as an intelligent and purposeful leader, committed to principles more consistent than heretofore recognized. Taft's libertarian philosophy was based on the principles of equal justice under law, equality of opportunity, and human liberty. In foreign affairs he was motivated by faith in the example of American liberty before the world and by a consistent desire to limit United States commitments overseas. By demonstrating the consistency of these ideals Patterson has succeeded in bringing intellectual order to Taft's actions and beliefs. Yet, as the author admits, the senator was frequently "flexible." Clearly this flexibility often reflected partisan necessity of the moment, responsiveness to his urban-industrial Ohio constituency, and/or essential

maneuvering for his next quadrennial run for the presidency. Though Patterson avoids saying so, his own impressive evidence presents a strong case for Taft as a skillful politician. To admit this would detract nothing from Taft's historical reputation; that Patterson does not do so reflects his adherence to the common American tendency to view excellence and political skill as mutually exclusive.

There are few weaknesses, however, in this impressive study. If Patterson tends occasionally to take subtle and gratuitous swipes at certain of Taft's rivals (notably John W. Bricker, Thomas E. Dewey, and Dwight Eisenhower), the author explains exceptionally well Republican factionalism in the 1940s and 1950s. His analyses of the 1948 and 1952 conventions, particularly, suggest the interesting thesis that personal and geographical rivalries had at least as much to do with such factionalism as did ideological difference. Unfailingly attentive to the historical context and significance of his subject, Patterson describes Taft's life with intelligence, sensitivity, and grace. If the dual objectives of political biography are to evoke and explain the man and his times, then *Mr. Republican* is a model of its genre.

GARY W. REICHARD
Ohio State University

ATHAN THEOHARIS. *Seeds of Repression: Harry S. Truman and the Origins of McCarthyism*. Chicago: Quadrangle Books. 1971. Pp. xi, 238. \$6.95.

How informative are book-jacket blurbs? The front flap of *Seeds of Repression* begins, "In this provocative new interpretation of the McCarthy years in America, Athan Theoharis lays the major responsibility for the Senator's rise at the doorstep of the White House during the Truman administration. He convincingly argues that President Truman helped create a political climate which made possible the McCarthy phenomenon in the early 1950's." The back flap predicts that the book "is certain to revise historical thinking and raise new questions about Harry S. Truman's accomplishments as President of the United States." *Caveat legens*: The book is provoking rather than provocative, unconvincing in its simplistic and poorly documented circumstantial case against Truman, and unlikely by itself to cause much revision of historical thinking.

First, McCarthy and McCarthyism are not the centerpieces of the book, and the reader ought not expect to learn very much about Senator McCarthy as a political personality or McCarthyism as a political force in American politics. They simply provide the author with symbols and a time frame to lay responsibility for the rise and intensification of the cold war on Truman. The assertion that Truman and his associates, by igniting and politically feeding upon fears of Soviet Communist expansion abroad and subversion in the United States, seeded a national climate of fear and suspicion which gave McCarthy his issue and made McCarthyism politically profitable is only instrumental to the primary theme that Truman broke with FDR's policy of diplomacy and detente with the Soviet Union and replaced it with an aggressive rhetoric but more moderate policy of saber-rattling abroad and witch-hunting at home.

Second, the book is unidimensional in its analysis. Theoharis sees American motives, actors, and actions, and he sees American images of Soviet motives, actors, and actions. But he does not incorporate their Soviet counterparts into his analysis of cause (origin) or assignment of blame. One need not be Hardhat Joe to recognize that the Soviet Union was more than a "bogie" manufactured by Truman and McCarthy; that there was a reality of Soviet motives (ideology, interests, strategy), actors (remember Stalin?), and actions; and that these have a bearing on sound historical interpretation of the era. The book would have been better informed and balanced, and more convincing, had Theoharis examined studies of Soviet domestic politics and foreign policy in this period, sought to capture the multifaceted dimensions of the decade's domestic and international politics, and provided the evidence and pauses that permit and invite the reader to make an independent judgment.

Third, Theoharis neither understands nor likes politics. The book is not informed or integrated by a coherent or realistic concept of politics. He accuses Truman of "oversimplified moralism" in his cold-war postures. But the author himself portrays the dramas of politics as morality plays and not complex scenarios of crisscrossing and conflicting currents of interests and ambitions in which mar-

ginal gains often are the results of massive efforts and skills of accommodation. Theoharis states, "A politician rather than a statesman, Truman was more attuned to short-term requirements than long-term results." Good grief! He contrasts FDR's "sophisticated" approach of negotiation and detente with the Soviet Union to Truman's reliance on "the primacy of power in international politics." Perhaps Truman, in the context of *his* presidency, was also "sophisticated." Arthur Schlesinger, jr.'s recent recollection in *Atlantic Monthly* of a Henry Wallace quip about FDR would apply equally well to Truman: He "could keep all the balls in the air without losing his own."

Fourth, Theoharis explains the absence in the text of source citations and the interpretations of other scholars by his hope to reach a "nonacademic audience." If his aim is to persuade the nonacademic reader that Truman was the principal creator of an unnecessary as well as cruel cold war (and McCarthyism), with its confrontations abroad and Red hunts at home, then ironically its impact will be just the opposite. The nonacademic reader will conclude from *Seeds of Repression* that Truman was not tough enough with Stalin abroad and his "agents" in Washington, and that McCarthy was right, after all. Theoharis, by presenting a one-dimensional and therefore exaggerated argument, presses his reader full circle in terms of its final verdict.

Finally, the sentence on Harry S. Truman, as it relates to the cold war and its internal security and foreign-policy components, and to his stature as president, which no doubt will be mixed, will, one hopes, necessarily rest upon more thorough, penetrating, and balanced scholarship.

HERBERT WALTZER
Miami University

CONSTANCE MCLAUGHLIN GREEN and MILTON LOMASK. *Vanguard: A History*. (The NASA Historical Series.) Washington: Scientific and Technical Information Division, Office of Technology Utilization, National Aeronautics and Space Administration. 1970. Pp. xvi, 308. Paper \$3.00.

CONSTANCE MCLAUGHLIN GREEN and MILTON LOMASK. *Vanguard: A History*. Washington: Smithsonian Institution Press. 1971. Pp. xvi, 308. Cloth \$12.50.

This book takes its title from the name of that first entry by the United States in the space age rivalry. It is the history of the program that developed and launched the Vanguard sequence.

For the many persons who will have forgotten these events, let it be said again that rockets first commanded attention as the awesome weapon in the hands of the German army. Postwar developments in Russia and the United States built on this German foundation, as both nations strained to perfect the intercontinental missile. Space exploration was not rated as significant in this weapons race.

There were individuals in the military services and, of course, in the universities and scientific societies who were both interested and active. The creation of the National Science Foundation in 1950, financed with federal funds, and the acceptance of the International Geophysical Year (IGY) in 1952 allowed the development of plans for space exploration with purposes beyond the military. Not until 1955 was further funding available.

Despite the insistence that IGY was not secret and was scientific only, the undertone of rivalry with Russia ran through this program. Representatives of that country had announced Russia's entrance into a satellite program just five days after the American announcement. Americans had been told about, though few had noted, Russia's activity. Americans were not prepared for Sputnik.

The sight of that tumbling object, launched on October 4, 1957, produced close to panic. The press was hysterical, congressmen demanded action, and in the excitement the achievements of the American scientists were ignored. The first result was the breaking down of restrictions, which to date had limited military participation, to allow a joint crash program, and by midsummer the United States had not one but two satellites. More important was the enactment, in July 1958, of legislation providing for planned, continuing activity with national decision-making authority concentrated in NASA.

This volume, which gives essential background, is basically a history of the years 1955-58 and is itself a critically important background for studying NASA. The technical problems that the scientists faced were com-

plex; they involved agreement on the program's purposes, instrumentation, vehicle, launching site, and tracking system, with decisions interrelated. The effort of the authors to translate all this into the language of the layman was quite successful. The efforts to untangle the alphabetical agencies, bureaus, and committees and to describe the competitive bureaucratic ways in and around government were probably as difficult. The thoughtful student can learn much here on how government functions.

The first author, Mrs. Green, is the distinguished historian of Washington, D.C., and of several of its governmental institutions. Mr. Lomask is a writer and teacher in Washington whose special contribution to this volume is those sections dealing with field tests and the Cape. Charles A. Lindberg has contributed an informed and thoughtful foreword, reflecting in part his participation in some of these councils and committees. Dated in 1969, his addition still carries the touch of skepticism about press and politics that must have been widely shared by scientists in the fifties.

The Smithsonian Institution Press has given the volume an attractive dress, with generous and well-chosen illustrations and charts.

RAYMOND C. MILLER

Wayne State University

JOSEPH C. GOULDEN. *Meany*. New York: Atheneum. 1972. Pp. 504. \$12.95.

This biography of George Meany is a superior piece of journalistic history. Produced at breakneck speed—two years from conception to publication for a meaty 500-page book must be some kind of record—Joseph C. Goulden's account inevitably suffers from the defects of the genre. The background is established from little more than textbook knowledge; the evidence is not carefully sifted and weighed (T. Harry Williams's *Huey Long*, which likewise depends heavily on oral sources, makes an instructive comparison here); and there is not much reflection on the underlying issues. But the virtues far outweigh the defects in this case, partly because of Goulden's skill and energy, even more so because of the nature of his subject. It would probably be impossible to write a first biography of George Meany in any other

way. The AFL-CIO president has (as Goulden observes) "assiduously avoided paper all his life" (p. 469). Even had the written record been fuller, this would not have adequately revealed the sources of Meany's stature within the labor movement nor his potency in national politics. No one but a working journalist could have developed Goulden's sure grasp of the realities of power and influence, the limits as well as the open ground, that explain Meany's role in American labor since World War II. Goulden was indefatigable in interviewing Meany, normally an inaccessible figure, and a long list of other union officers and staff men, many of whom demanded anonymity. To a large extent, in fact, Goulden's biography has the character of an oral history, but several cuts above the average, because Goulden was able to elicit pointed, detailed information usually lacking in the memoirs in oral history archives.

Goulden has given us a shrewd and informative biography of George Meany. It advances our present understanding of the modern labor movement that stems from the old AFL—its political evolution beyond Gompers's nonpartisanship (with Meany as Exhibit A), its cold-war mentality and consequent adventures (financed mostly by the U.S. government) into international politics, and the broadening out from tight craft unionism (from which Meany sprang) to accommodate to the demands of industrial unionism and New Deal social policy. The book will have a second career: it will provide labor historians with a treasure of oral history that otherwise probably never would have been committed to paper.

There is one cause for regret: the book ends in 1971 and so deprives us of an inside history of Meany's part in the 1972 election.

DAVID BRODY
University of California,
Davis

LOUIS M. KOHLMEIER, JR. *"God Save This Honorable Court!"* New York: Charles Scribner's Sons. 1972. Pp. x, 309. \$8.95.

This volume illustrates an old saw that most books should be articles. The core of the study is a perceptive account of the struggle to control appointments to the Supreme Court during the Johnson and Nixon administrations. But

the author, a reporter for the *Wall Street Journal*, wastes almost one-third of the space in unsuccessful efforts to place these events in historical context and the rhetoric of crisis. The result is an uneven work whose quality improves the shorter the historical sight.

The chapters devoted to setting the stage are too flat and oversimplified to serve as useful popular history. Despite an occasionally dramatic detail, the author misses the ebb and flow of judicial power in American history and the complex roles that judges play in the modern polity. Assuming racial equality to be the only issue that counts, he not only underestimates such tragic figures as Roger B. Taney and Abe Fortas, but the author overestimates the "Supreme Court crisis" provoked by President Nixon's Southern strategy. Nowhere is it demonstrated that Nixon appointees have forsaken minority rights or their own independence to the crisis point. Nor are facts faced that the Warren Court was losing public support and that court-packing is a classic method of harmonizing federal courts with popular tolerances.

It is fortunate that the author ignores his backdrop when recounting recent conflicts over Supreme Court succession. Though the coverage of the Fortas, Haynsworth, and Carswell nominations overlaps other popular accounts, Kohlmeier's treatment of the Carswell episode is fairer than Richard Harris's *Decision* (1971). The best chapter covers the appointments of Justices Powell and Rehnquist after the American Bar Association Standing Committee on the Federal Judiciary opposed Richard H. Poff, Herschel H. Friday, and Mildred L. Lillie for want of professional distinction. Kohlmeier argues effectively that these three candidates were neither decoys nor coin to demean the Supreme Court, as some observers feared, but rather the entrapment of Mr. Nixon in his own political criteria. Certainly the same criteria made G. Harrold Carswell's selection predictable.

A contemporary chronicle is rarely final. Though flawed by oversimplification and needless padding, this account of the Johnson and Nixon nominations to the Supreme Court illuminates the role of the American Bar Association in the politics of judicial selection and long-standing tensions among political and

professional standards for service on the nation's highest court.

J. WOODFORD HOWARD, JR.
Johns Hopkins University

CANADA

HAROLD MARTIN TROPER. *Only Farmers Need Apply: Official Canadian Government Encouragement of Immigration from the United States, 1896-1911*. Toronto: Griffin House. 1972. Pp. 192. \$8.95.

Immigration to Canada in the first decade of the twentieth century is generally thought of in terms of the wave of Southern and Eastern European peasants who literally flooded the vast reaches of the Canadian west. *Only Farmers Need Apply* deals with a lesser known but equally important immigration, that of over half a million American citizens and residents who crossed the border in search of cheap and fertile land. The subject has been examined before, notably by Paul Sharp and Karel Bicha; Harold Troper's contribution is a study of the organizational structure of the federal government's immigration branch, which was assigned the task of encouraging American farmers to come to Canada. Thoroughly researched and generally quite readable, this monograph unfortunately suffers from a lack of rigorous editing and is still too obviously a dissertation. Several chapters, for example, those describing the activities of government agents in various American communities, are needlessly detailed and include a good deal of relatively insignificant material. More effective are those in which the content of the publicity and the increasingly restrictive policies of the immigration branch are outlined. Troper shows how the branch's campaign was consciously designed to fit into the tradition of American land promotion. Americans were expected to understand that the prevailing image of western Canada as a frozen wasteland was because of the kind of misrepresentation the American West had suffered when it was known as the "Great American Desert." At a time when the supply of cheap land in the United States appeared to be running out, the Canadian west was pictured as the "Last Best West," really a continuation of the mov-

ing American frontier. Yet it was advertised as a continuation with a difference. North of the border was an orderly and peaceful society, by implication a sharp contrast to the supposedly lawless Western American society.

As Troper demonstrates, however, the border was not considered open to all Americans. Canada was looking for farmers, and, in particular, white farmers. No others need apply. The myth that racial discrimination existed only south of the border has recently been challenged by Robin Winks in his history of the blacks in Canada, and Troper supplies additional evidence by describing how westerners pressured the immigration branch to close the border to Negroes. As a result the large-scale immigration from the United States during this period apparently included very few blacks.

Although the scope of this study is purposely limited to the function of a government agency, Troper reminds us of questions that remain concerning the nature and magnitude of this immigration. Perhaps the most important of these is the significance of this immigration in the shaping of western Canada.

GILBERT A. STELTER
University of Guelph

LATIN AMERICA

FRIEDRICH KATZ. *The Ancient American Civilizations*. Translated by K. M. LOIS SIMPSON. (History of Civilization.) New York: Praeger Publishers. 1972. Pp. xvi, 386. \$15.00.

The title of this book may mislead the average reader. Professor Katz has written an account of the Indian high cultures of the central Andes and Meso-America from their origins to the coming of the Europeans. The organization of the book reflects the dual areal base, for after a unified consideration of more general questions—such as sources for studying the pre-Conquest cultures, the origins of the American Indians, the much-vexed question of autochthonous invention of cultural traits, as against reception through diffusion—Professor Katz presents his analysis in parallel chapters. The major divisions of time for the two areas are essentially the same: formative or preclassical, classical, postclassical or urban or militaristic.

In both the central Andes and Meso-America, two remarkable Indian structures emerged in the fifteenth century, to have their courses truncated by the Spaniards in the sixteenth.

The entire book is the product of judicious reflection by an able mind fully abreast of a widely dispersed literature. Katz reviews, perhaps at too great length, old questions such as the origins of the American Indians and agriculture among them, but his basic interest is in the role of technology, religion, militarism, and similar clusters of traits in promoting change and in the nature and direction of the change. The function of irrigation in providing both social surplus for the support of upper classes and creating the need for a strong state apparatus (the Wittfogel thesis) is found especially true for the desert coast of the central Andes. In Meso-America, irrigation was important to a far lesser extent; the unique chinampa provided much of the production for urban life. The Maya with their advanced intellectual achievements resting upon an apparent exiguous base remain a puzzle. The framework for understanding that Katz brings to phenomena serves here only to emphasize how fragmentary and contradictory is our present knowledge.

Perhaps most of all Katz is interested in the movement toward larger, more complex social and political structures in possible parallel development to the Old World. The most original part of his book lies in his examination of the Triple Alliance and the Inca Empire in such terms. The Aztecs, he points out, ruled over a population held to heavy tribute but with little change in their socio-political structures; the Incas integrated subject peoples into the fabric of an essentially unified state. Yet Meso-America was in rapid change, and the question remains what would have emerged had not the Europeans annexed it with catastrophic rapidity to the Iron Age.

WOODROW BORAH
University of California,
Berkeley

BILLY JAYNES CHANDLER. *The Feitosas and the Sertão dos Inhamuns: The History of a Family and a Community in Northeast Brazil, 1700-1930*. (Latin American Monographs, Second

Series, number 10. Center for Latin American Studies, University of Florida.) Gainesville: University of Florida Press. 1972. Pp. xi, 178. \$7.50.

The perennial question as to whether New York City fairly represents the whole United States has its counterpart in the argument that no one knows the real Brazil if his experience is limited to the *café* and beaches. Mr. Chandler agrees, and *The Feitosas* is a useful corrective. This book also partially satisfies the lament of the brilliant historian J. H. Plumb, who writes that "historians have not studied family life very closely." But Chandler's work satisfies only in part, for it is totally unlike our family counseling books, and his "family" is more like the Medici or the Du Ponts or a Mafia family than the ever-provocative Loud family. The Feitosas do not all bear that last name, and the *parentela* includes clients and loyal employees, who all take part in the recurring *lutas* between power groups. They stand for whatever law there is in a remote and lawless region until, from time to time, the central government can infuse national standards or a rival family wins its way to the top. The government is forced to rely largely on the local VIP's and their autocratic power; outside authority is, or rather was, seldom exercised.

Chandler's monograph makes full use of Brazilian documents and historical writing—with the startling exception of Da Cunha—rather than the innovative techniques of the late Oscar Lewis.

This "narrative local history" begins with the rigorous natural characteristics of this part of Ceará and the original Portuguese land grants. It does not attempt to do much with national history, but it covers the efforts made to eliminate the Indian threat and the problem created by Negro slavery. This is part of the Brazilian reality, and Chandler does not claim that it is typical even of all isolated and poverty-stricken areas down to 1930.

The law of the books was less important than the unwritten law and private hate and vengeance. We, too, have examples of the nullification of law by the jury. Schools seldom functioned and the Church was insufficiently staffed. The droughts, or *secas*, occurring at intervals of less than ten years, caused the

death of many animals and the departure, temporary or permanent, of the *retirantes* (withdrawers), sometimes as much as ninety per cent of the population, and the destruction of a viable economy. In spite of all this, the author concludes with a recognition of the value of a society that is much more traditional than ours and less addicted to change. His treatment of this society is vastly informative and judicious.

W. REX CRAWFORD
University of Pennsylvania

EDGAR GABALDÓN MÁRQUEZ. *El México virreinal y la "Sublevación" de Caracas, 1810*. (Biblioteca Venezolana de Historia, 13.) [Caracas:] Archivo General de la Nación. 1971. Pp. 576.

News of the discovery and publication of documents bearing upon an important historical event (as contained in the prologue of this work) is exciting. And, indeed, Gabaldón Márquez traveled to Mexico to study and photocopy an unedited file of documents consisting of 308 folios, which describe measures taken by the viceregal government of New Spain in reaction to the Caracas "rebellion" of April 10, 1810. The measures were secret because the authorities of New Spain wished to conceal the events in Venezuela from the Mexican population in order to prevent the "contagion" from spreading. The viceroyalty instructed officials under its jurisdiction to censor news and mail and to impose an embargo against the infected province, and it appealed to English authorities in Jamaica to take similar steps in order to help contain the revolt. The prophylactic measures that the viceregal government took were vigorous. However, Gabaldón Márquez does not present these facts in an orderly fashion, and one needs a great deal of patience in order to extract the information.

The book is essentially a philosophical-historical essay in which the author muses upon numerous thoughts that the documents and events provoke in his mind, the principal ones being secrecy in government ("Secrecy is a reflection of fear—fear on the part of a small group that it will lose advantages acquired at the expense of mankind" [p. 81]) and imperialism ("Is there anyone today who thinks

that Moby Dick . . . is a symbol of evil; no, absolutely not. . . . Moby Dick represents human liberty, pursued by the Ahabs, the whale-hunters, the hard-hearted profit-seekers, the island-hunters, the exploiters of other men, and the mortal enemies of the splendid whale-creatures" [p. 461]).

The author is erudite, wide-ranging, and amusing, but he also rambles and makes parenthetical statements within parenthetical statements. Like Melville, whom he admires, he is easily diverted. When he discusses the arrival of the news in Vera Cruz of the uprising in Caracas, he feels compelled to describe conditions in general in New Spain and to survey the prevailing knowledge there about Venezuela. When he treats the efforts of the viceroyalty of New Spain to suppress the news of the Caracas revolt, he adds a chapter about measures taken earlier in reaction to the Haitian revolt. And when he describes the appeals to the English in Jamaica, he writes at length about the English presence in the Caribbean and is carried away by his Anglophobia. This is legitimate, of course, but the work fails to fulfill the promise of the prologue and seems inappropriate for a publication of the Archivo General de la Nación. In short, the work is too eclectic for its own good.

CHARLES D. AMERINGER
Pennsylvania State University

WILLIAM F. SATER. *The Heroic Image in Chile: Arturo Prat, Secular Saint*. Berkeley and Los Angeles: University of California Press. 1973. Pp. viii, 243. \$10.50.

Early in the War of the Pacific, Chile lost one of her decrepit warships, which was then under the command of Arturo Prat. In the clash Prat not only lost his ship but his life as well when he, along with only one other sailor, leaped upon the deck of the enemy vessel in a futile gesture to inflict some damage upon the Peruvians before the battle was lost. Prat became a hero to the Chileans and remains to this day one of his country's foremost historic figures. By examining attitudes toward Prat expressed in print over the subsequent seventy years the author of this book seeks to explain how and why a defeated commander becomes a great hero to his countrymen.

Prat's image shifted with events in Chile. During the remainder of the war he was used either as an antigovernment symbol, when some blamed military failures on political leaders, or as a means to strengthen morale when the population despaired of victory. Once the war ended successfully for Chile, the press and political and intellectual leaders displayed a lack of interest in the fallen hero. But when political problems developed and a civil war ravaged the nation in 1891, Prat was resurrected in the press. Following the civil war the new government was accused of corruption, and Prat was now used to emphasize virtue and morality. Later he became the inspiration for patriotism and nationalism and, as problems divided the nation, the symbol for national unity. At times political parties also made use of the naval hero. In the 1930s the Nazi party praised him and the socialists used him to dramatize the failures of the government. The military recalled Prat in its publications as higher budgetary appropriations were sought. Meanwhile, educators tried to instill in their students the qualities of self-sacrifice, duty, and discipline that ennobled Prat. In all of this the hero was immortalized not for his military action but rather for the human

qualities he displayed in battle, qualities that the nation hoped to re-establish.

Basing his study largely on the newspaper and periodical treatment of Prat and the Battle of Iquique, Sater succeeded in linking Prat's fame to political and social developments in Chile. When there was need for an example to dramatize some failing in the nation, Prat was hauled out to be held up for emulation. The hero in this case became the vehicle for propaganda.

This study, unfortunately, makes little note of the press treatment accorded other heroes such as Bernardo O'Higgins. Such a comparison and analysis would have placed the Prat phenomenon in a larger and perhaps more meaningful context. Nevertheless, this is an excellent book. The thesis is set forth precisely and logically amid succinct sketches of Chilean history that place Prat in his role as hero in proper perspective. The book is, in addition, exceptionally well written. The author obviously devoted a considerable amount of effort to the literary aspect of his history with the result that literary quality and scholarship are united in a most pleasant fashion.

JACK RAY THOMAS

Bowling Green State University

Communications

A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

TO THE EDITOR:

The Halsted Press Division of John Wiley and Sons, Inc. would like to inform your readers that V. D. Nabokov's *The Provisional Government*, which was reviewed in the April 1974 *AHR* (79: 546-49), has been withdrawn from sale in the United States.

WARREN SULLIVAN
President, Halsted Press

TO THE EDITOR:

In his article "Naturalism and Socialism in Germany" (*AHR*, 79 [1974]: 14-37), Vernon L. Lidtke holds the rigidity of Marxist theory accountable for the failure of German nat-

uralists to affiliate with the Social Democratic party in Imperial Germany. While this may be a valid judgment in respect to Otto Brahm, founder of the *Freie Bühne*, it is hardly an accurate assessment in the case of writers such as Karl Bleibtreu, author of the manifesto *Revolution der Literatur* (1885) and spokesman for the "Youngest Germany," or Michael Georg Conrad, founder of the Munich-based journal *Die Gesellschaft*. Professor Lidtke underestimates the nationalistic and antimaterialistic bias of the early naturalists. These writers sought to generate a literature commensurate with the grandeur of the German Empire. In Zola they admired the iconoclast who had successfully defied an outworn literary tradition in France. Bleibtreu and Conrad embraced naturalism as a means of overcoming the blandness of popular literature and the formalism of an older generation of writers. Description of social reality offered a means to shock the philistine out of his complacent materialism and indifference to art.

The concern of these writers with the social question in no way meant sympathy with the labor movement. They did not pose the problem in terms of how to correct economic imbalances but rather in terms of how to reduce the bitterness of class relations. In their view class warfare resulted from the materialistic attitudes to which both worker and bourgeois subscribed. They implicitly rejected the labor movement as one of the instigators of social conflict. Only a new idealism—a revival of heroic spiritual values shared by all—could in their view provide an adequate solution to the social problem.

To have accommodated Bleibtreu, Conrad, and the "Youngest Germany," the S.P.D. would

have had to cease being socialist in all but name, a condition met by the "national socialist" parties both before and after the First World War.

RODERICK STACKELBERG
University of Massachusetts,
Amherst

PROFESSOR LIDTKE REPLIES:

Roderick Stackelberg grossly oversimplifies the argument in my article and compounds his unfortunate misunderstanding by simply disregarding the evidence presented to demonstrate that many naturalists held views antithetical to Social Democracy. Stackelberg raises the case of Michael Georg Conrad with the suggestion that the article underestimates his nationalism and antimaterialism. This is a strange error for Stackelberg to make for I specifically pointed out that Conrad was a nationalist and that he vigorously attacked the most fundamental principles of Social Democracy, including materialism and internationalism. I did not and would not recommend that the Social Democrats should have accommodated themselves to a man like Conrad by abandoning their Marxism and adopting his chauvinistic beliefs. That would have been absurd. The issue is not the rigidity of Marxism, as Stackelberg wrongly maintains, but Marxism's intellectual superiority over anything the naturalists could produce and the resulting confidence this theoretical supremacy gave to the Social Democrats.

Stackelberg falls into other errors of oversimplification. Preoccupied with Conrad and Bleibtreu, he generalizes as if they spoke for the whole of the naturalist movement. That is hardly the case, as he must certainly know. The naturalist movement had its own diversity. Moreover, it should be emphasized that the social views of many naturalists underwent changes and did not remain static as Stackelberg seems to assume. If one takes into account both the personal diversity and the developmental factor then it is easier to understand the complexities of the relationship between socialists and naturalists.

VERNON L. LIDTKE
Johns Hopkins University

TO THE EDITOR:

I do not wish to quibble over the motives behind American intervention in Cuba. I stick to the viewpoint set forth in my work, *The Spanish-Cuban-American War and the Birth of American Imperialism, 1895-1902*, despite the criticism in the review by Kenneth J. Hagan (*AHR*, 79 [1974]: 240-41). But since Professor Hagan asserts that I did not pay attention to the desire of the United States to assist Cuba in achieving independence from Spain, I would like to point out that I devote an entire chapter in the first volume ("The American People and Cuban Independence") to demonstrate that the majority of the American people distinctly favored and supported Cuban independence and that this desire was utilized by the McKinley administration to frustrate that independence. In the second volume I demonstrate the opposition in the United States to the Platt Amendment on the grounds that it deprived Cuba of any meaningful independence. If Professor Hagan wishes to identify the "Americans" as represented solely by the McKinley administration and other imperialists in this country, that is his privilege.

Professor Hagan also asserts that "racism" pervades my "discussion of American anti-imperialists," leaving the impression that I attribute the anti-imperialist impulse solely to the fear of adding more nonwhite population to the American nation. This is not borne out by the evidence in the books. It would, moreover, be strange that, having published a laudatory analysis of Mark Twain's anti-imperialist writings (see *Mark Twain: Social Critic* [1958]), I would view the anti-imperialist movement so narrowly. But the fact is that racism was deeply imbedded in American anti-imperialism at the turn of the century, although some anti-imperialists who started out as racists learned from their anti-imperialist activities how dangerous this view was to the welfare of their own country. Perhaps Robert L. Bessner put it best in his *Twelve Against Empire: The Anti-Imperialists, 1898-1900* ([1968], pp. 232-33): "They [the anti-imperialists] can be condemned as racists. Whether they used words like 'superior' and 'inferior' or 'civilized' and 'uncivilized,' they thought of peoples in the categories of racism. George

Hoar was generally an exception, but even he gave himself over to this kind of thinking on occasion. The unhappy fact is that few Americans were immune to the prevailing racism of the late nineteenth century. The anti-imperialists were no better or worse than their countrymen."

PHILIP S. FONER
Lincoln University

PROFESSOR HAGAN REPLIES:

As I indicated in my review, Professor Foner's book is an impressive piece of research that lucidly traces the struggle in Cuba. My major reservation about the two volumes is that they interpret annexationists as being motivated almost exclusively by desire for material gain. Professor Foner advanced a similar thesis in volume 2 of an earlier work, *A History of Cuba and Its Relations with the United States* (1963). He argues that during the 1880s "the pernicious influence of big business" was pervasive in American government. He believes that "scarcely a week passed without some public disclosure of advantages and lawless concessions [being] granted to corporations, passed into law by bribed legislators, signed by corrupt executives, and approved by judges who were subservient tools of the corporate interests." In this environment Secretary of State Blaine's proposal for reciprocal trade was not a "liberal trade policy." It was merely "a device for the United States to dump surplus products on the Latin American market, and dominate that market for North American economic interests."

Corporate leaders did have great influence upon the United States in the late nineteenth century, but American foreign policy was more than a conspiracy to gain wealth through increasing exports. A widely held contemporary opinion contended that unless the United States increased its exports there might well be a social upheaval brought on by angry workers suffering the effects of reduced income as a result of decreased demand for industrial goods. Many Americans who had no personal vested interest in industrial exports therefore wanted to increase the volume of those shipments. Ultimately they accepted the economic rationale for imperialism.

The tendency to oversimplify interpretations

is also apparent in *The Spanish-Cuban-American War* when Professor Foner dismisses an analysis of American policy makers' fear of German penetration of the Caribbean as superficial. Yet, as Richard Challener and others have recently shown, throughout the period from 1900 to 1914 American naval strategists considered Germany the most likely enemy and the Caribbean the most probable theater of operations in any war involving the United States and Europe.

Similar points might be made about the racial attitudes of the late nineteenth century. Professor Foner's citation of *Twelve Against Empire* is most pertinent. But the fact that anti-imperialists held racist assumptions does not diminish the validity of their fear for the constitutional integrity of their country as it abandoned precedent and embarked on an overseas imperial adventure.

It is my hope that this exchange will spark a heightened interest in Professor Foner's work and a testing by others of my criticism. I look forward to a great deal more discussion of *The Spanish-Cuban-American War*.

KENNETH J. HAGAN
United States Naval Academy

TO THE EDITOR:

After criticizing a number of details, Gordon L. Davies, in his review of my book *Charles Lyell. The Years to 1841: The Revolution in Geology* (*AHR*, 78 [1973]: 1461-62), writes: "I have been forced to conclude that Wilson understands neither the true nature of Lyell's geological beliefs nor the scientific milieu within which he operated."

To support such a sweeping assertion Dr. Davies offers only the criticism that I praise Lyell for his work on the origin of river valleys in Auvergne in 1828. When Lyell and Roderick Murchison were in Auvergne in May 1828 they made observations which demonstrated conclusively that the valley of the Sioule River was excavated, not just once, but repeatedly, over a long period of time by the water flowing in the river itself. Their observations destroyed the contrary theory of the Reverend William Buckland that the river valleys of Auvergne had been formed on a single occasion by the waters of a great deluge which had swept the whole country. Inasmuch as Lyell held in 1828 that

the Auvergne River valleys were formed by the rivers flowing in them, his ideas were in agreement with the earlier theories of Hutton and Playfair. As Lyell's ideas developed he attributed influence also to the action of waves along lines of seacoast in creating land forms. On page 378 of my book I describe Lyell's view in 1833 of the origin of the land forms of the Weald and the Chalk Downs. Lyell's and Murchison's work on river valleys was, however, a minor part of their work in Auvergne. Lyell was much more concerned with the Tertiary freshwater formations of Auvergne, with the picture that their fossils gave of conditions in the lakes of Auvergne during the geological past, and with the Auvergne volcanoes.

Does Dr. Davies himself understand Lyell's geological beliefs? He criticizes my failure to mention in greater detail Lyell's observations in 1818 of the effects of a flash flood in the Val de Bagnes. I do mention (pp. 62-63) Lyell's observations of the vast amount of sand brought down by this flood and add that Lyell "would always be aware that the occasional recurrence of such violent events as floods was part of the regular order of nature and helped to produce geological change." Dr. Davies refers to the flood of the Dranse as a "catastrophic debacle." It was, however, not a catastrophic debacle in the sense that the word "catastrophe" was understood in the nineteenth century—that is, an event of a magnitude many times greater than any geological change occurring in the world today. Although the flood of the Dranse was disastrous to the inhabitants of the Val de Bagnes it was an example of geological forces at work.

Dr. Davies simply ignores my extensive discussion of the development of Lyell's geological thought and creates the impression that my book gives only a picture of Lyell's everyday life. The supercilious tone of Davies's review and his contemptuous dismissal of the work of another are not only unjustified but are also incompatible with civilized historical discourse.

LEONARD G. WILSON
University of Minnesota

TO THE EDITOR:

Since my late collaborator, Professor Edwin H.

Zeydel, is no longer in a position to defend himself against Allen Cabaniss's superficial and unfair review of our translation of Einhard's *Vita Karoli Magni: The Life of Charlemagne* (*AHR*, 78 [1973]: 1035), I feel called upon to defend our work most vigorously. Some of Mr. Cabaniss's criticisms can be dismissed easily, for he has plainly chosen to ignore what is clearly printed in our book for everyone to see and read: thus, our bibliography is intentionally called a "Selected Bibliography" and for this reason lists "only thirty-six books and articles" chosen from a flood of historical scholarship on Einhard and Charlemagne. As we expressly state in our preface, moreover, we have chiefly included recent German scholarship on Einhard and Charlemagne. Our footnotes, similarly, can hardly be called "thin," since they comprise almost twenty pages and elucidate a text only a little more than twice as long (forty-five pages). Whatever Mr. Cabaniss's editorial theory may be, it was not our intention to bury Einhard in footnotes.

As to Mr. Cabaniss's observation that "several debatable statements occur in the introduction," I should like to know of any scholarly book of which the same cannot be said. As a historian he ought to know that history is composed of "debatable statements," and that the study of history dies when the debate ceases. If, however, by "debatable" Mr. Cabaniss means "inaccurate" then he should have the courtesy of proving his point. Of his two so-called illustrations, the first—referring to an "apparent conflation" of Louis the Pious and Louis the German—apparently refers to the statement that "under Louis the Pious, it [the Imperial title] merged with a concept of Frankish unity, the sovereignty over all other hereditary Frankish rulers. But Louis the German was an opponent of the imperial office, so Einhard, looking to the future, perforce bowed to his feelings" (pp. 24-25). If this is indeed the passage to which Mr. Cabaniss refers, then the conflation must exist in Mr. Cabaniss's mind alone, since the two Louis's are clearly distinguished from each other. As to his second point, that we are wrong to claim that "in the eighth and ninth centuries marriages did not involve the Church," this is one of those debatable points on which any historical interpretation must be based. What Mr. Cabaniss should note in his review is that his

suggestion that marriages at this time did involve the Church is just as debatable. Furthermore, in the context it is clear that what is being presented here is a summary of historical scholarship and not an original interpretation on the part of the translators.

I object strongly to this shoddy treatment of a carefully thought-out introduction. But most strongly I object to Mr. Cabaniss's off-hand remarks about the quality of our translation. He does not seem to be aware of the difference between a translation and a trot. For readers such as himself we have provided the Latin text which can be glossed *ad libitum*. (My German translation, by the way, has received an excellent critical press and has been reprinted several times by Reclam.) It is easy enough to find deviations of a word-for-word kind in any good translation, and, indeed, word-for-word accuracy can be the most unfaithful way of bringing a literary text from one language into another. I do not wish to discourse here at length on the theory of translation, but it is my hope that this letter will provide me and my late collaborator—as well as innumerable translators who have been maltreated by reviewers as superficial as Mr. Cabaniss—with at least some, if still inadequate, redress for the damage done to us.

EVELYN SCHERABON FIRCHOW
University of Minnesota

TO THE EDITOR:

One stands aghast at the irresponsibility of Bernard S. Bachrach's review of my book, *A Jewish Principdom in Feudal France 768–900* (*AHR*, 78 [1973]: 1440–41).

Bachrach offers a totally inaccurate summary of my method. I based my conclusions on all available and relevant eighth- and ninth-century records, not on "a highly selective use of *chansons* and other literary materials from the twelfth century and later."

For example, the epistle of Pope Stephen III of 768, responding "anguished to the point of death" to information supplied by Bishop Aribert of Narbonne, is not a later "fictional literary source(s)," in Bachrach's characterization. The bishop advised the pope that the Frankish kings had granted extensive territories in free allod to the Jews, in fulfillment of a

pledge. Stephen declares the kings can nullify their pledge since God Himself withdrew His promises to the Jews in consequence of the Crucifixion. What pledge? I draw the answer from ninth-century documents and later sources, which, I show, are based on Carolingian materials.

Thus, the thirteenth century *Gesta Karoli Magni* describes the Jews' surrender of Narbonne in return for a pledge of the Frankish king for a ruler of their own. Parts of the *Gesta* are based on older historical materials. I show that Pope Stephen's epistle complements and completes the *Gesta*. To Bachrach this means basing conclusions on "fictional literary sources."

I locate Carolingian fragments in the twelfth-century *Addendum* to the Hebrew *Sefer haKabbalah*. The writer describes the seal and briefly summarizes the contents of a capitulary issued by a "King Carolus" to the Jews of Narbonne, to be dated 791. This *Addendum* also reports a royal cession of hereditaments to Narbonne Jewry. It discloses the name of the scholar-prince, a scion of King David and former exilarch in Baghdad, who arrived in Frankia by royal invitation and established a dynasty of Jewish princes.

Bachrach charges that I "abandoned the canons of historical method." In fact, I demand that such canons be applied to the documents of the Carolingian age, especially when they derive from ecclesiastical sources and involve Jews. The ninth-century Goth monk who compiled the *Annals of Aniane* attributed to Goth residents of Narbonne its surrender to the Franks for a pledge to have their own law. Seventy-five years ago Pückert warned that this monk falsified fact with astounding audacity. I emphasize the incongruities in this text and the contradiction with historical reality, since Pepin banished the Goths on acquiring Narbonne but granted vast allodial lands to the Jews. To Bachrach such a method is "to accept as superior the evidential value of fictional literary sources . . . and to relegate documentary evidence of a more contemporary nature to inferior status."

I have searched for authentic documentation regarding Jews in numerous contemporary sources as, for example, the ninth-century *Poème* of Ermold. I demonstrate that Ermold's chronology for the siege of Barcelona is based on

the Jewish calendar. This successful assault involved Jewish forces led by their own prince.

ARTHUR J. ZUCKERMAN
City College,
City University of New York

PROFESSOR BACHRACH REPLIES:

While no one would question Rabbi Zuckerman's right to stand "aghast," the question of "irresponsibility" will remain for our readers to decide. Zuckerman is not the first nor will he be the last partisan of Jewish history to be misled by papal hyperbole or to be seduced by the theory of Christian conspiracy. The "Goth monk" and those other "ecclesiastical sources" that sought to deprive Saint William of his Davidic heritage have not fooled Zuckerman, because in the later Middle Ages various fictional sources relate the truth. William and his family had long noses, William ate no pork, and had several wives. Perhaps some learned Cadi in one of our emerging ethnic programs will soon honor us with a monograph entitled "An Arab Kingdom in the Carolingian Empire."

BERNARD S. BACHRACH
University of Minnesota

The following letters have been received in connection with the publication of John A. Garraty's "The New Deal, National Socialism, and the Great Depression," AHR, 78 (1973): 907-44.

TO THE EDITOR:

I would like to claim some space to comment briefly on John Garraty's recent article on the Nazi and New Deal response to the depression of the 1930s. While I suspect that others will want to comment on what I consider some of the strained comparisons he makes between Germany and the United States, this is not the reason I take issue with the article. Like Professor Garraty I have no objection to anyone's recognizing good in the Nazis, so it is not from his revisionist attitude that my objections arise. Rather it is his deliberate use of the so-called comparative method that seems to call for comment. An examination of Garraty's article

can reveal quite clearly, I think, the limitations as well as the advantages of cross-national comparisons.

Professor Garraty's article makes evident that it is when differences are uncovered that comparison makes its principal contribution to our understanding or analysis of the past. It is true that a cross-national comparison which reveals similarities increases our understanding. Usually what we learn is that a given phenomenon had similar results in two or more different societies, suggesting further that the two societies are not as different as we had hitherto been led to believe. Thus the uncovering of similarities can correct erroneous assumptions about the differences between two countries or societies. But in this case Professor Garraty has been quite specific in denying that he is showing that the New Deal was more fascistic than we thought or that the Nazi regime was more democratic. In fact, at the beginning of his article he states his purpose as an attempt to "demonstrate that Nazi and New Deal antidepression policies displayed striking similarities. Since the two systems, seen in their totality, were fundamentally different, these similarities tell us a great deal about the depression and the way people reacted to it." The proposition, I believe, is a non sequitur, for if the two societies are "fundamentally different" what do the similarities reveal to us that we could not have learned without a comparison between Germany and the United States? The evidence which Professor Garraty adduces, for example, to show that the Nazis had a concern for the unemployed or that they used propaganda to advance their antidepression policies did not require a comparison with the New Deal. Nor was a comparison of Germany and the United States necessary in order to show that Hitler and Roosevelt were charismatic leaders.

My point, however, is not that Garraty's article is without contribution. The point rather is that his contribution to our understanding comes only when he moves away from a comparison between Germany and the United States and looks, however briefly, at the depression in England and France. Then the German and American comparison spring into life, telling us "a great deal about the depression and the way people reacted to it." For now we learn that not all societies reacted to the depression

in the same way, and that those which differed may have been of the same political order. Thus political democracies like England and France had a quite different response, as Professor Garraty shows, from that in another political democracy, the United States. In sum, the comparison with Germany did not tell us anything about the depression in the United States and the reaction to it that we could not have learned better from a comparison with England or France. About all, on a conceptual level, the German comparison seems to add to our understanding is that the differences between the response of the New Deal and the French Popular Front, for example, could not be ascribed to the differences between a New World and a European country.

My object here, however, is not to suggest the various hypotheses one might fruitfully examine, on the basis of Professor Garraty's comparative matrix of four countries, but rather simply to point out that differences, not similarities, are the significant elements in cross-national comparisons of this sort. Consequently, I would respectfully submit that Professor Garraty would have done us a much greater service if he had elaborated his English or French comparison and reduced, if not eliminated, his German. Then, I think, some rather striking avenues for future research and study in regard to the impact of the depression would have been opened up. The German comparison as it worked out suggests only one, and that one has been excluded by the author from the outset—namely, that the Nazis and the New Deal were more alike than we had suspected.

CARL N. DEGLER
Stanford University

TO THE EDITOR:

I found John Garraty's article fascinating. I agree with the similarities he finds between Hitler's Germany and Roosevelt's America and would also accept his comparisons and contrasts with Great Britain. I do not feel, however, that the pages devoted to Léon Blum are quite as good. In the first place, I noticed no

reference to the fact that Blum was Jewish. To be a Jewish président du Conseil in France in 1936 was probably a greater handicap than being Austrian-born (Hitler), having a Harvard accent (FDR), or being old (Chamberlain).

More important is the factor of the interplay of France's parliamentary system and the multiplicity of parties. Let us not speak of Hitler who, once in power, destroyed all legal opposition and only had to fear a coup d'état. Roosevelt, elected in 1932, knew he had four full years and, after 1936, that he had four more years. True, Chamberlain was responsible to Parliament, but British parties are usually well disciplined and he could presumably count on Tory support for five years. In fact, due to the war, the Parliament elected in 1935 lasted ten years. Admittedly he could be and was eventually replaced by another Conservative.

Now let us look at Blum. He was at the head of one of three parties in a coalition. Professor Garraty has himself shown that Blum's own socialists were divided. In addition, he had to keep simultaneously the support of the Radical Socialists (who, as most people know, were neither radicals nor socialists) and the Communists. Perhaps Blum should have tried for more profound reforms but his position was not easy: contrary to FDR, Hitler, and Chamberlain, Blum might be overthrown any day in the Chamber or in the Senate. The loss of either of his coalition parties would be fatal. In addition, Blum did not want to be a Kerensky, always a possibility. I would dispute Garraty's sentence: "Yet Blum's efforts were pitifully inadequate, no better or worse than those of uninspired premiers who preceded and followed him." I wonder, incidentally, how many AHA members could even name the premier immediately before Blum in 1936 or his successor in 1937.

On page 938, Mr. Garraty says the franc was worth about five cents in 1935. If my memory is correct, one got fifteen francs for the dollar, which would come to nearly seven cents for a franc.

BERNARD SINSHEIMER
*University of Maryland,
European Division*

Recent Deaths

J. SALWYN SCHAPIRO, emeritus professor of history at the City College of New York, died in New York on December 30, 1973, at the age of ninety-four. Born December 19, 1879, at Hudson, New York, he was graduated from City College in 1904. He returned to the college as a tutor in history and served there for forty years until 1947, when he was awarded the City College Townsend Harris Medal for outstanding alumni achievement. He received his doctorate from Columbia University in 1909.

Schapiro's first scholarly book was his dissertation, *Social Reform and the Reformation*, which appeared in 1909, the year he was promoted to instructor. From then on a series of scholarly works and textbooks flowed from his pen. His *Modern and Contemporary European History* was for several decades the most popular textbook in its field and dominated the national market. *Liberalism and the Challenge of Fascism* (1948), one of his finest works, emphasized the great and lasting values of the liberal state. His two paperbacks, *Liberalism: Its Meaning and History* (1958) and *Movements of Social Dissent in Modern Europe* (1962), were perennial best sellers. *The World in Crisis: Political and Social Movements in the 20th Century* (1950) was widely quoted as a model of historical analysis.

He had a deep-rooted sense of social justice. At the age of eighteen he was a founder of Madison House, a New York City settlement house at the Downtown Ethical Culture Society under the leadership of Dr. Henry Moskowitz and others. Schapiro's aim was "to welcome the refugees and the immigrants for whom the Gay Nineties were less than gay, and to strengthen the home ties between immigrant parents and American-bred children."

In 1934 Schapiro was elected head of the City

College chapter of the American Association of University Professors. During World War II he worked for the war department's historical service. From 1939 to 1941 he was acting chairman of his department. He was a visiting professor at Columbia University, the New School, the Universities of Southern California, Colorado, and Montana, and the North Carolina College for Women. He was an active member of the board of editors of the *Journal of the History of Ideas*.

Schapiro was the true intellectual. A gentleman, rich in intellect, a scholar who combined erudition with compassion, he was a rare human being who had a strong impact on teachers and students alike. His friendship was firm and giving. He had no ulterior motives. All who knew him agree that he was—unconsciously to be sure—the apotheosis of the great academician.

LOUIS L. SNYDER
City College,
City University of New York

CLIFFORD KENYON SHIPTON knew more about the lives and thoughts of eighteenth-century Harvard graduates than any man who lived at that time. In a little over forty years he wrote fourteen volumes of *Biographical Sketches of Those who Attended Harvard College* in which he revived the memory of more than 2,000 men from the classes of 1690 through 1771. Born August 5, 1902, in Pittsfield, Massachusetts, he was a member of the Harvard class of 1926. In 1930 he was engaged by Samuel Eliot Morison to continue for the Massachusetts Historical Society this biographical work which John Langdon Sibley had begun in 1859. Shipton received his Harvard Ph.D. in 1933. Although

he held other posts that would have been full-time occupations for an ordinary man, he continued work on this project for the rest of his life. Through great industry in research, combined with a beguiling literary style, he constantly brought the obscure dead to life. Any volume of Shipton's Sibley is as good bedside reading as John Aubrey's lives. Happily Shipton was more prolific than his seventeenth-century predecessor. For readers without access to the whole series, the Harvard University Press in 1963 published an anthology of "Shipton's Lives" under the title *New England Life in the 18th Century*.

In 1938 he was put in charge of the Harvard University Archives, a post that he continued to fill after he went to Worcester in 1940 as librarian of the American Antiquarian Society. He was director of that society from 1959 until he retired in 1967. At Worcester, aside from being responsible for many other important bibliographical works, he edited 166 boxes of microcards that reproduced all American imprints through the year 1800. In 1967-68 he was president of the Society of American Archivists, and from 1961 until November 1973 president of the Colonial Society of Massachusetts. Although he retired from the Harvard Archives in 1969, he was working there on a Sibley biography the day before his death on December 4, 1973.

In addition to Sibley and his great bibliographical works, Shipton found time to write

biographies of Roger Conant and Isaiah Thomas, to take an active part in the town affairs of Shirley, Massachusetts, and to be endlessly helpful to his colleagues and students. He received an honorary doctorate of letters from Harvard in 1965 and from Clark University in 1969.

WALTER MUIR WHITEHILL
Boston Athenaeum

Other members of the association who have died recently include: Andre A. Beaumont of New York; Francis Benjamin of Emory University in Atlanta, Georgia; Donald Bradeen of the University of Cincinnati, Ohio; James Brewer of the University of North Carolina in Chapel Hill, North Carolina; Ralph Burton of Detroit, Michigan; Catherine Crary of Scarsdale, New York; Florence Crofut of Hartford, Connecticut; Charles A. Culotta of Bryn Mawr College, Pennsylvania; T. J. Curry of Atlanta, Georgia; C. P. Foulke of Philadelphia, Pennsylvania; Paul M. Kendall of Lawrence, Kansas; Kathryn J. Lee of Chattanooga, Tennessee; Harold Manakee of Baltimore, Maryland; Rev. Charles H. Metzger, S. J., of Colobiere College in Clarkston, Michigan; Amory O. Moore of Fort Atkinson, Wisconsin; Edward H. Phillips of Charleston, South Carolina; E. S. Phinney of Joplin, Missouri; Donald H. Sheehan of Whitman College in Walla Walla, Washington; and John Paul Yoder of Doylestown, Pennsylvania.

Association Notes

For the past twenty-five years Professor Leland H. Carlson of the University of Southern California has compiled the British Commonwealth and Ireland list for the Recently Published Articles section of the *AHR*. Professor Carlson recently retired and will no longer serve as a section editor. Because of his skill and dedication, the *AHR* has provided comprehensive bibliography for an important and broad field of study. The *AHR* staff and the profession owe a debt of gratitude to Professor Carlson for his outstanding service.

Mrs. Janet Hearne, who has served as an assistant editor of the *AHR*, has been appointed editor of the *AHA Newsletter*, replacing Mrs. Janet Hayman, who has resigned from that position. Miss Maryann Lesso is joining the staff of the *AHR* as an editorial assistant.

Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

BLANE, ANDREW, editor, and BIRD, THOMAS E., associate editor. *Russia and Orthodoxy: Essays in Honor of Georges Florovsky*. Vol. 3, *The Ecumenical World of Orthodox Civilization*. The Hague: Mouton. 1974. Pp. 250. 80 gls.

GEORGE H. WILLIAMS, Baptismal Theology and Practice in Rome as Reflected in Justin Martyr. HARRY A. WOLFSON, The Identification of *ex nihilo* with Emanation in Gregory of Nyssa. T. F. TORRANCE, The Relation of the Incarnation to Space in Nicene Theology. JOHN MEYENDORFF, Free Will (γνώμη) in Saint Maximus the Confessor. PETER CHARANIS, Church-State Relations in the Byzantine Empire as Reflected in the Role of the Patriarch in the Coronation of the Byzantine Emperor. WILHELM KAHLE, Die Tränen der Frommen in der Gottesbegegnung (Ein Beitrag zur oekumenischen Spiritualität). ERNST BENZ, Sophia—Visionen des Westens. ROBERT STUPPERICH, Protestantismus und Orthodoxie im Gespräch. STEVEN RUNCIMAN, The British Non-Jurors and the Russian Church. YVES M.-J. CONGAR, O.P., Église de Pierre, Église de Paul, Église de Jean: Destin d'un Thème Oecuménique. N. A. NISSIOTIS, An Orthodox View of Modern Trends in Evangelism. C. J. DUMONT, O.P., La Levée des Anathèmes de 1054 (7 Décembre 1965) et sa Signification dans la Conjoncture Oecuménique Contemporaine. CHARLES MOELLER, Nouveaux Aspects de L'Oecuménisme.

FRITZ, PAUL, and WILLIAMS, DAVID, editors. *City & Society in the 18th Century*. Toronto: Hakkert. 1973. Pp. 301. \$12.00.

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sannes d'Ancien Régime. TAMÁS HOFFMANN, Faillite et culture de la paysannerie (Hongrie—XIX^e siècle). IMRE KATONA, L'organisation et les communautés temporaires des ouvriers saisonniers instables en Hongrie (1848-1945). PIERRE BARRAL, Le monde agricole français vers 1950. PÉTER GUNST, L'agriculture hongroise dans la première moitié du XX^e siècle. MAGDA SOMLYAI, Le partage des terres. LÁSZLÓ KOMLÓ, Révolution agraire et industrialisation du complexe alimentaire hongrois. VICTOR-L. TAPIÉ, Résultats et conclusions.

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A GUIDE TO THE ABBREVIATION OF JOURNAL TITLES

GENERAL RULES: Proper names are spelled out with the exception of adjective forms and names of countries used to identify place of publication. Whenever possible, articles, conjunctions, and prepositions are deleted. Exceptions are relatively short titles such as *Past & Present* or *The Americas*.

<i>abt</i>	Abteilung	<i>bol</i>	boletim, boletín
<i>acad</i>	academia, academy	<i>boll</i>	bollettino
<i>accad</i>	accademia	<i>brandenburg</i>	brandenburgisch
<i>adm</i>	administration, administrative	<i>bras</i>	Brasil, brasileira, Brazilian
<i>aff</i>	affairs	<i>braunschw</i>	braunschweigisch
<i>afric</i>	africain, African, Africana, africanum	<i>Braz</i>	Brazilian
<i>afrik</i>	afrikaanse	<i>brem</i>	bremisches
<i>agrar</i>	agrarisches	<i>Brit</i>	British
<i>agric</i>	agricultural, agriculture	<i>bull</i>	bulletin
<i>AHR</i>	<i>American Historical Review</i>	<i>bus</i>	business
<i>aikakausk</i>	aikakauskirja	<i>byz</i>	byzantine
<i>akad</i>	Akademie		
<i>Ala</i>	Alabama	<i>cah</i>	cahiers
<i>Alas</i>	Alaska	<i>Calif</i>	California
<i>alemann</i>	alemannisches	<i>Can</i>	Canada, Canadian
<i>allg</i>	allgemein	<i>Carib</i>	Caribbean
<i>altertumsk</i>	Altertumskunde	<i>cath</i>	catholic
<i>alttest</i>	alttestamentliche	<i>cent</i>	century
<i>Am</i>	American, Americana, Amerikas	<i>cercet</i>	cercetări
<i>an</i>	anales, annales, annals, annali, annals, annua, annuaires, annual, annuarium, anuarul	<i>českoslov</i>	československý
		<i>chron</i>	chronicles, chronique
<i>anc</i>	ancien, ancient	<i>circ</i>	circle, circular
<i>annot</i>	annotation, annotator	<i>civil</i>	civilization
<i>anthol</i>	anthologica, anthology	<i>class</i>	classica, classical, classique
<i>anthropol</i>	anthropological, anthropologie, anthropology	<i>co</i>	county
<i>antiq</i>	antiquarian, antiquarisch, antiquarischen, antiquité, antiquities, antiquity	<i>coll</i>	college
		<i>collect</i>	collection, collections
<i>antol</i>	antología	<i>Colo</i>	Colorado
<i>antropol</i>	antropologiczny	<i>com</i>	comité, committee
<i>anz</i>	anzeiger	<i>comm</i>	commerce
<i>appenzell</i>	appenzellische	<i>comp</i>	comparate, comparative, comparée
<i>arch</i>	archiven, archives, archivio, archivo, archiv, archivum	<i>compil</i>	compilation, compiled, compiler
<i>archaeol</i>	archaeolog, archaeologia, archaeology	<i>concl</i>	conclusion
<i>archäol</i>	archäologie, archäologische, archäologischer	<i>conf</i>	conference
<i>archeol</i>	archeologia, archeologica, archeologie, archeology	<i>cong</i>	congress
<i>Ariz</i>	Arizona	<i>Conn</i>	Connecticut
<i>Ark</i>	Arkansas	<i>cont'd</i>	continued
<i>ark</i>	arkiv	<i>contemp</i>	contemporaine, contemporánea, contemporary
<i>arq</i>	arquivos		
<i>arqueol</i>	arqueológico	<i>contrib</i>	contributed, contribution, contributor
<i>art</i>	article	<i>corp</i>	corporation
<i>assoc</i>	association	<i>corr</i>	correspondence
<i>assyriol</i>	assyriological, assyriologie, assyriology	<i>c. r.</i>	comptes rendus
<i>at</i>	atti	<i>crit</i>	critica, criticism
<i>Atl</i>	Atlantic	<i>cuad</i>	cuaderno
<i>AUMLA</i>	<i>Journal of the Australasian Universities Language and Literature Association</i>	<i>cult</i>	cultura, cultural, culture
<i>autobiog</i>	autobiography		
		<i>D.C.</i>	District of Columbia
<i>b</i>	buch (compounds only)	<i>Del</i>	Delaware
<i>balt</i>	Baltic, baltisch	<i>demog</i>	demografie, demographische, demography
<i>bayer</i>	bayerisch	<i>Den</i>	Denmark
<i>Beitr</i>	Beitrag, Beiträge	<i>dept</i>	department
<i>Ber</i>	Bericht	<i>deux</i>	deuxième
<i>bibl</i>	bibliotek, bibliotheca, bibliothèque	<i>dev</i>	developing, development
<i>bibliogr</i>	bibliografice, bibliographical, bibliography	<i>dig</i>	digest
<i>bijd</i>	bijdragen	<i>dipl</i>	diplomatic, diplomatique
<i>biog</i>	biography	<i>doc</i>	documentation, documents
<i>bk</i>	book	<i>dok</i>	dokuments
<i>bl</i>	Blatt, Blätter	<i>drev</i>	drevnei
<i>BMGN</i>	<i>Bijdragen en Mededelingen betreffende de Geschiedenis der Nederlanden</i>	<i>dtsch</i>	deutsche, deutschen, deutsches
		<i>e</i>	east, eastern
		<i>ec</i>	economics, economique, economy
		<i>eccles</i>	ecclesiastical
		<i>ecles</i>	eclesiástico
		<i>ed</i>	edited, edition, editor

<i>educ</i>	education	<i>int</i>	internacional, international, internationale, internazionale
<i>EEH</i>	<i>Explorations in Economic History</i>	<i>interdisc</i>	interdisciplinary
<i>EHR</i>	<i>English Historical Review</i>	<i>intern</i>	internal
<i>ELH</i>	<i>English Literary History</i>	<i>introd</i>	introduced, introduction
<i>Eng</i>	English	<i>ist</i>	istorii, istorijski, istoriski
<i>epig</i>	epigraphik, epigraphy	<i>istruz</i>	istruzione
<i>Epis</i>	Episcopal	<i>ital</i>	Italian, italiana, italienisch
<i>estud</i>	estúdios		
<i>et</i>	études		
<i>ethnog</i>	ethnographisch	<i>j</i>	journal
<i>ethnol</i>	ethnological, ethnology	<i>jb</i>	Jahrbuch, Jahrbücher
<i>etnol</i>	etnologia	<i>JEGP</i>	<i>Journal of English and Germanic Philology</i>
<i>Eur</i>	Europäische, Europas, Europe, European, européennes	<i>jugoslav</i>	jugoslovenski
<i>ev</i>	evangelisch	<i>jur</i>	juridical, juridiceski, juridique
<i>explor</i>	explorations		
		<i>kan</i>	kanonistisch
<i>fac</i>	faculté, faculty	<i>Kans</i>	Kansas
<i>facs</i>	facsimile	<i>kath</i>	katholik, katholisch
<i>fak</i>	Fakulte	<i>kd</i>	Kunde
<i>fil</i>	filosofía, filozofski, filozofskog	<i>kl</i>	Klasse
<i>filol</i>	filología	<i>Ky</i>	Kentucky
<i>Fla</i>	Florida		
<i>for</i>	foreign	<i>La</i>	Louisiana
<i>forsch</i>	Forschung, Forschungen	<i>landesk</i>	Landeskunde
<i>fr</i>	français, France, French	<i>lang</i>	language
<i>francisc</i>	franciscanos, franciscanum	<i>lett</i>	letter, letterário, letteratura, letterature, lettre
<i>fränk</i>	fränkische	<i>lib</i>	library
<i>frankf</i>	frankfurter	<i>Lib Cong</i>	U.S. Library of Congress
<i>fries</i>	friesisches	<i>libr</i>	librarian
		<i>ling</i>	linguistics, linguistique
<i>g</i>	giornale	<i>lit</i>	literary, literatur, literature, literatury, littérature
<i>Ga</i>	Georgia	<i>lübeck</i>	lübeckische
<i>gaz</i>	gazette	<i>lüneburg</i>	lüneburger
<i>gen</i>	general, général		
<i>geneal</i>	genealogy	<i>mag</i>	magasin, magazine
<i>geog</i>	geografi, geográfico, geographic, geographical, géographique, geographischen, geography	<i>marit</i>	maritime
	German	<i>Mass</i>	Massachusetts
<i>Ger</i>	German	<i>Md</i>	Maryland
<i>germ</i>	germanistisch	<i>Me</i>	Maine
<i>ges</i>	Gesellschaft	<i>med</i>	medieval, médiévale, medievals, medievalia
<i>gesch</i>	Geschichte, geschichtliche	<i>meded</i>	mededelingen
<i>gos</i>	gospodarczych	<i>Mediterr</i>	Mediterranean
<i>govt</i>	government	<i>mél</i>	mélanges
<i>grad</i>	graduate	<i>mém</i>	mémoires, memorial, memorie
<i>grafsch</i>	Grafschaft	<i>mennonit</i>	mennonitische
		<i>Mex</i>	Mexican
<i>h</i>	hefte (compounds only)	<i>Mich</i>	Michigan
<i>hamburg</i>	hamburgisch	<i>mid</i>	middle
<i>hann</i>	hannoversche	<i>midcont</i>	midcontinental
<i>hell</i>	hellenic, hellénique, hellenistic	<i>mil</i>	militaire, militarisch, military
<i>helvet</i>	helvetian	<i>Minn</i>	Minnesota
<i>hess</i>	hessisch	<i>misc</i>	miscelánea, miscellany
<i>Hi</i>	Hawaii	<i>Miss</i>	Mississippi
<i>hisp</i>	hispanic, hispánicos, hispanique	<i>mitt</i>	Mitteilung, Mitteilungen
<i>hist</i>	histoire, historiae, historialinen, historical, historická, historický, histórico, historicum, historique, historisch, history, historyczne	<i>Mo</i>	Missouri
	hohenzollerische	<i>mod</i>	modern, moderna, moderne
<i>hohenzoll</i>	hohenzollerische	<i>mond</i>	mondiale
<i>holstein</i>	holsteinisch	<i>Mont</i>	Montana
		<i>monum</i>	monumenta
<i>iaz</i>	iazyka	<i>movim</i>	movimento
<i>Ida</i>	Idaho	<i>mt</i>	mountain
<i>Ill</i>	Illinois	<i>mus</i>	musée, musei, museum
<i>illus</i>	illustrated		
<i>ind</i>	industrial, industry	<i>n</i>	north, northern
<i>Inda</i>	Indiana	<i>nac</i>	nacional
<i>indivd</i>	individual	<i>nass</i>	nassauische
<i>inscr</i>	inscription	<i>nat</i>	national
<i>inst</i>	institut, institute, institution, instituto, institutului		

<i>nationalok</i>	nationaløkonomie, nationaløkonomisk	<i>rep</i>	report, reporter
<i>naz</i>	nazionale	<i>repub</i>	republic, republicii
<i>N.C.</i>	North Carolina	<i>res</i>	research
<i>N.D.</i>	North Dakota	<i>rev</i>	revolution
<i>ne</i>	northeast	<i>rhein</i>	rheinisch
<i>Nebr</i>	Nebraska	<i>R.I.</i>	Rhode Island
<i>neutest</i>	neutestamentliche	<i>ric</i>	ricerche
<i>Nev</i>	Nevada	<i>rocz</i>	roczniki
<i>newslett</i>	newsletter	<i>röm</i>	römische
<i>N.H.</i>	New Hampshire	<i>roman</i>	romanische
<i>niedersächs</i>	niedersächsisch	<i>roy</i>	royal
<i>N.J.</i>	New Jersey		
<i>N.M.</i>	New Mexico	<i>s</i>	south, southern
<i>no</i>	number	<i>S.C.</i>	South Carolina
<i>nos</i>	numbers	<i>Scand</i>	Scandinavia, Scandinavian
<i>Nor</i>	Norway	<i>sch</i>	school
<i>nord</i>	nordisk	<i>schles</i>	schlesisch
<i>norm</i>	normale	<i>schr</i>	Schrift
<i>numis</i>	numismatic, numismatique	<i>schweiz</i>	schweizerisch
<i>nw</i>	northwest	<i>sci</i>	science, scientiarum, scientific, scientist, scienze
<i>N.Y.</i>	New York	<i>S.D.</i>	South Dakota
<i>obit</i>	obituary	<i>se</i>	southeast
<i>oesterr</i>	oesterreichisch	<i>sec</i>	sectio, section
<i>ok</i>	økonomie	<i>ser</i>	série, series
<i>Okla</i>	Oklahoma	<i>slaw</i>	slawistik
<i>Ore</i>	Oregon	<i>soc</i>	social, societatis, society
<i>organ</i>	organization	<i>sociog</i>	sociographiques
<i>orient</i>	oriental, orientale, orientalia, orientalistyczny	<i>sociol</i>	sociologia, sociological, sociology
<i>österr</i>	österreichisch	<i>solothurn</i>	solothurnische
<i>osth</i>	Osthefte	<i>sozial</i>	sozialistischen
		<i>soziol</i>	Soziologie
<i>Pa</i>	Pennsylvania	<i>Span</i>	Spanish
<i>Pac</i>	Pacific	<i>spol</i>	Spółecznuch
<i>pädagog</i>	pädagogik, pädagogisch	<i>stat</i>	statistical, statistics, Statistik
<i>paedagog</i>	paedagogica	<i>stift</i>	stiftung
<i>pap</i>	papers	<i>stor</i>	storia, storici, storico
<i>papyrol</i>	papyrologie	<i>stud</i>	studi, studia, Studien, studies, studium
<i>parl</i>	parlementaire, parliament	<i>sup</i>	superiore
<i>pfälz</i>	pfälzische	<i>suppl</i>	supplement
<i>phil</i>	philosophical, philosophique, philosophy	<i>sw</i>	southwest
<i>philol</i>	philology	<i>Swed</i>	Sweden, Swedish
<i>photo</i>	photograph	<i>symp</i>	symposium
<i>pol</i>	political, politico, politics, Politik, politique, politische	<i>tech</i>	technisch
<i>pop</i>	popular	<i>technol</i>	technology
<i>port</i>	portuguesa, portuguese	<i>Tenn</i>	Tennessee
<i>pres</i>	president, presidential	<i>test</i>	testament, testamentum
<i>Presb</i>	Presbyterian	<i>Tex</i>	Texas
<i>preuss</i>	preussisch	<i>theol</i>	theological, theologie, theology
<i>probl</i>	problems	<i>tids</i>	tidskrift, tidsskrift
<i>proc</i>	proceedings	<i>tijd</i>	tijdschrift
<i>prot</i>	protestant, Protestantismus	<i>tr</i>	translated, translation, translator
<i>prov</i>	provence, provinces	<i>trans</i>	transactions
<i>PSQ</i>	<i>Political Science Quarterly</i>	<i>trav</i>	travail, travaux
<i>psych</i>	psychology		
<i>pt</i>	part	<i>u</i>	und
<i>pts</i>	parts	<i>U</i>	Universitătii, university
<i>publ</i>	publication, publishing	<i>unter</i>	Unterricht
<i>q</i>	quaderni, quarterly	<i>Va</i>	Virginia
<i>quel</i>	quellen	<i>vaterl</i>	vaterlandisch
<i>r</i>	review, revista, revue, rivista	<i>ver</i>	Verein, vereinigung, Vereins
<i>rass</i>	rassegna	<i>verh</i>	Verhandlungen
<i>rdsch</i>	Rundschau	<i>veröff</i>	Veröffentlichungen
<i>rec</i>	record	<i>vesn</i>	vesnik
<i>rech</i>	recherches	<i>vest</i>	vestnik
<i>regist</i>	register	<i>volksk</i>	volkskunde
<i>relig</i>	religieuse, religion, religiöse, religious	<i>vopr</i>	voprosy
<i>rend</i>	rendiconti	<i>vrem</i>	vremennuk
		<i>Vt</i>	Vermont

<i>w</i>	west, western	<i>yrbk</i>	yearbook
<i>Wash</i>	Washington		
<i>westf</i>	westfälisch	<i>z</i>	Zeitschrift, Zeitschriften
<i>wirtsch</i>	Wirtschaft, wirtschaftlich	<i>zeitgesch</i>	Zeitgeschichte
<i>Wis</i>	Wisconsin	<i>zgodov</i>	zgodovinski
<i>wiss</i>	Wissenschaft, wissenschaftlich	<i>zhurn</i>	zhurnal
<i>WMQ</i>	<i>William and Mary Quarterly</i>		
<i>württemb</i>	württembergisch		
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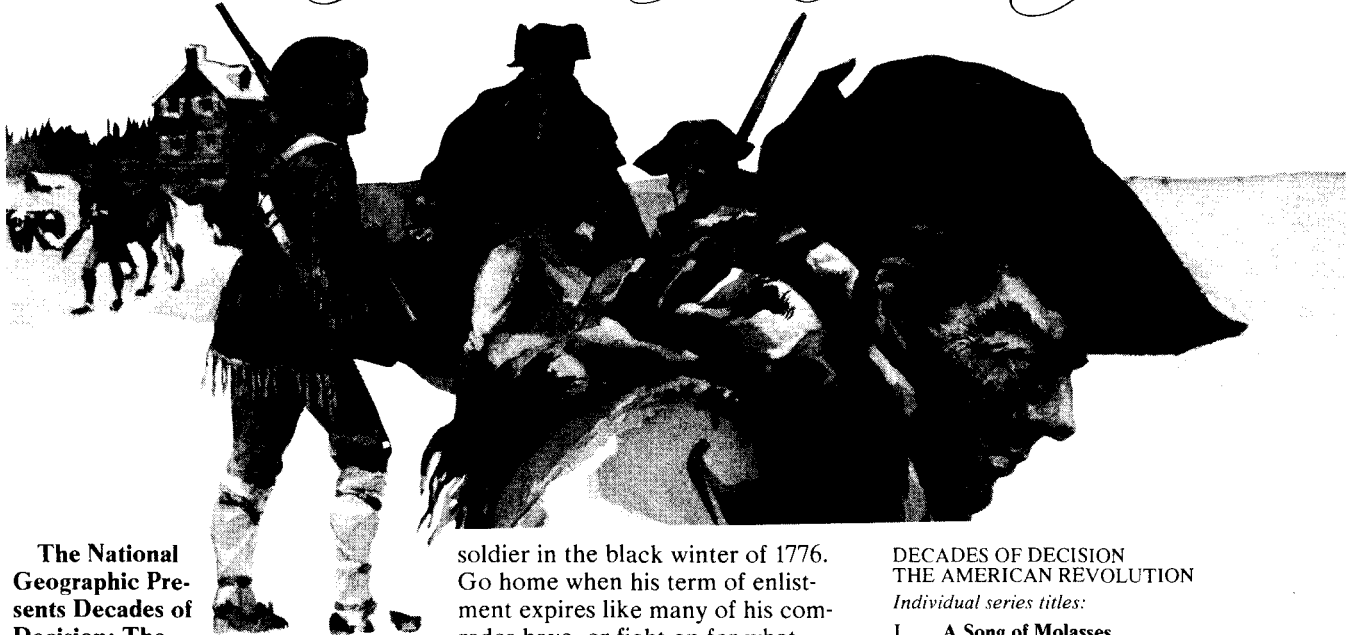
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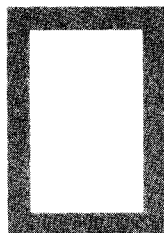
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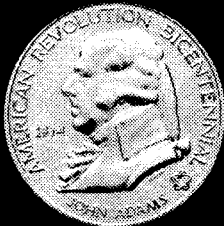
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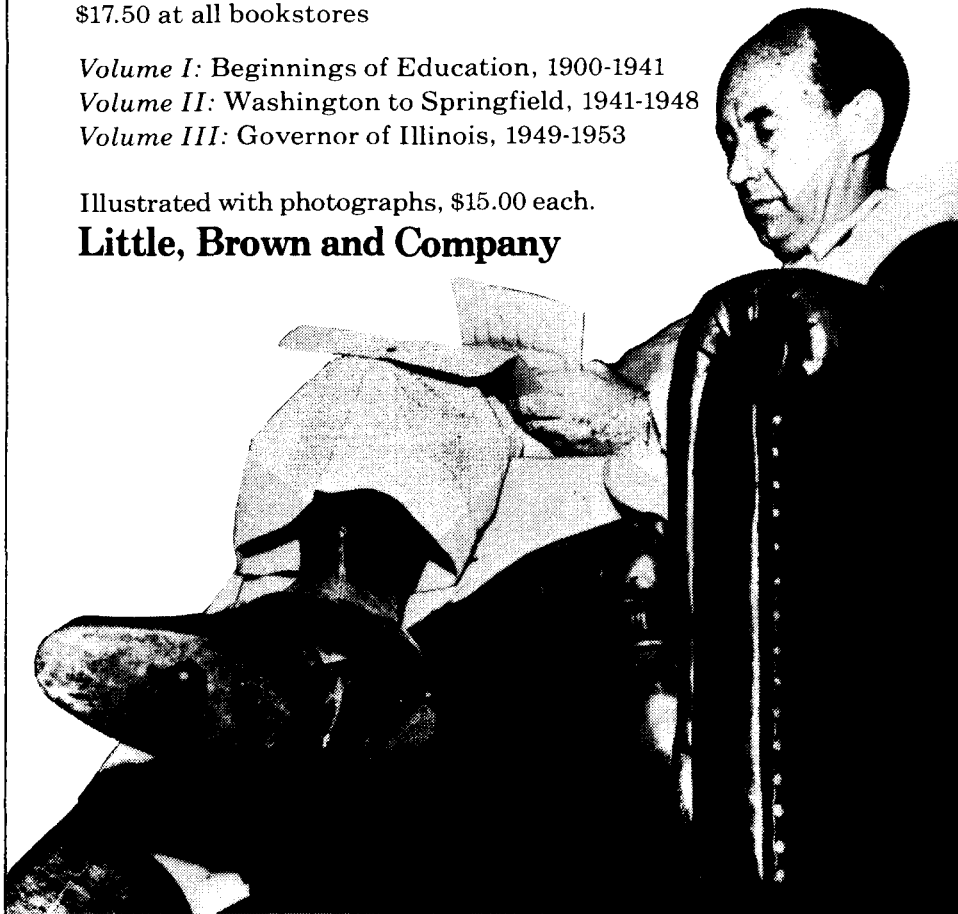
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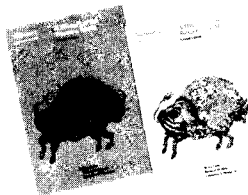
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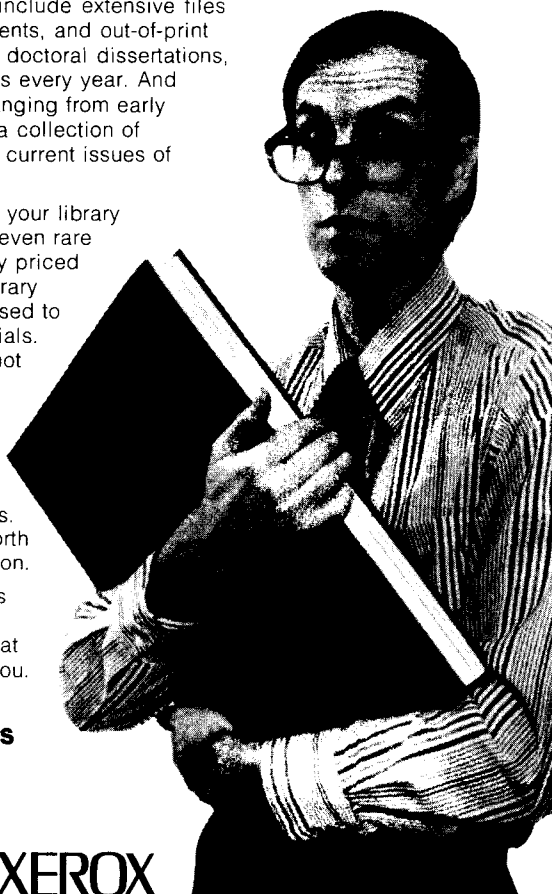
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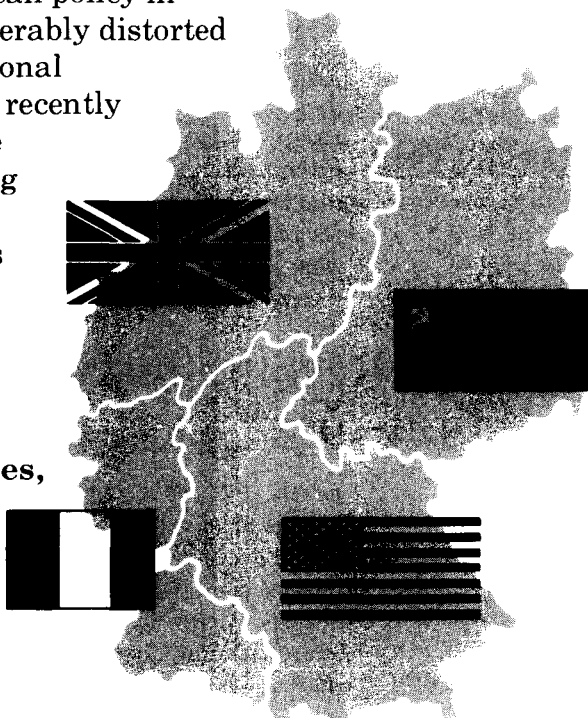
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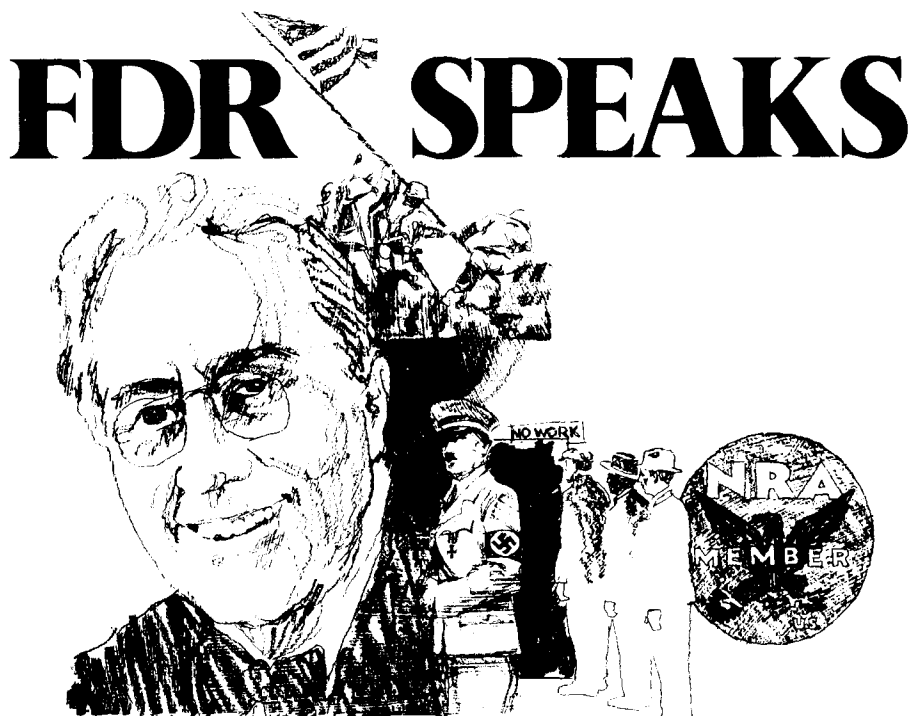
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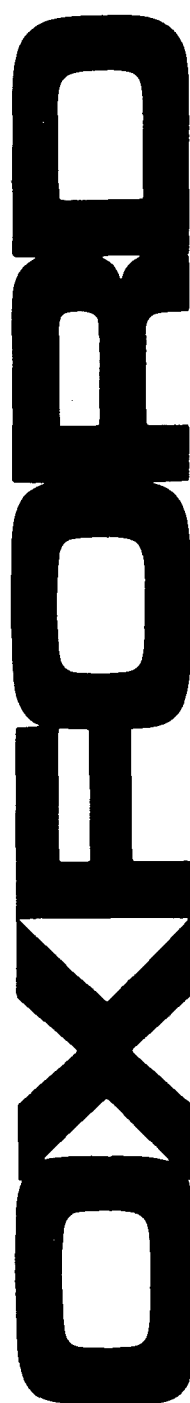
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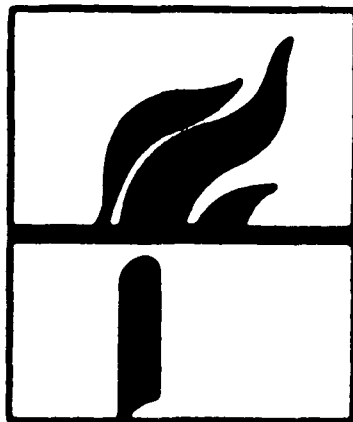
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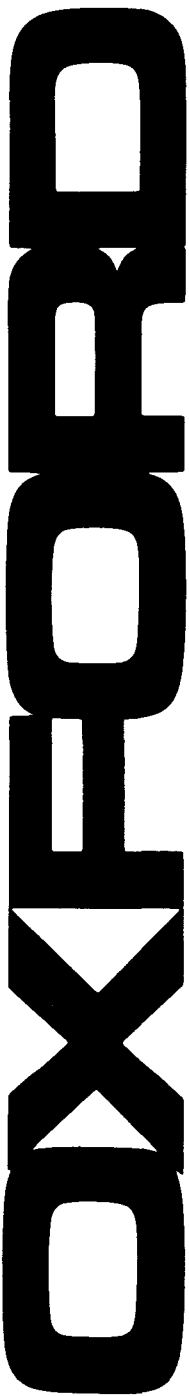
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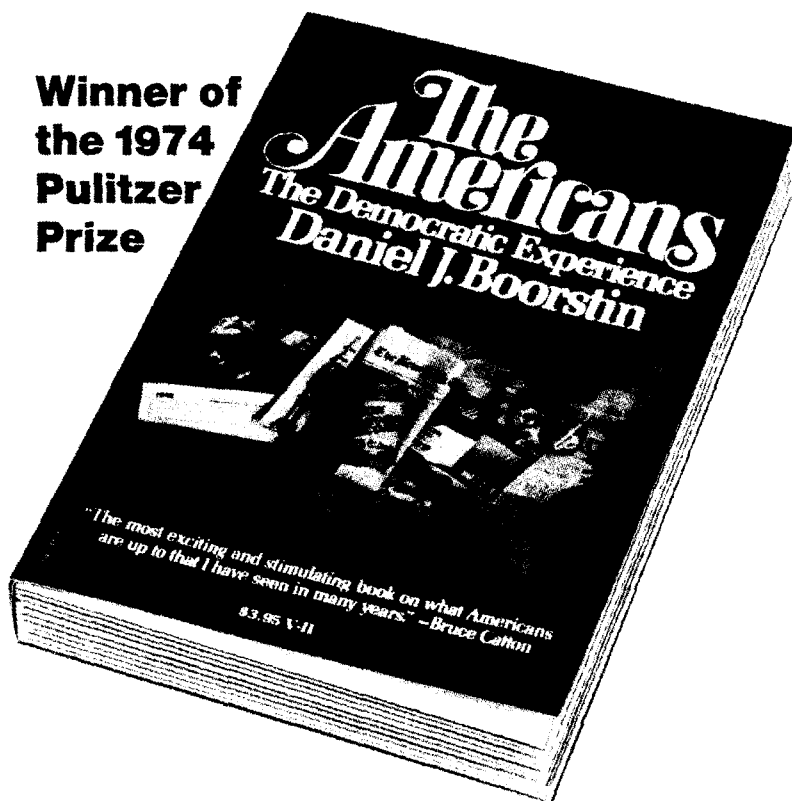
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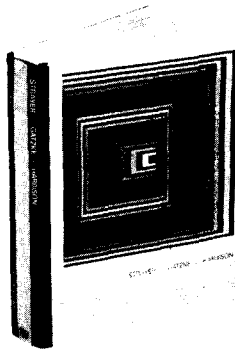
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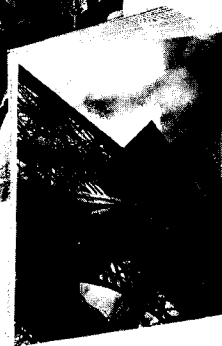
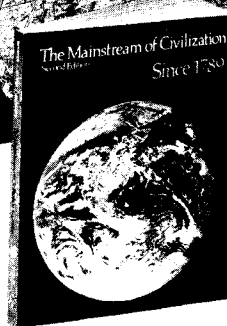
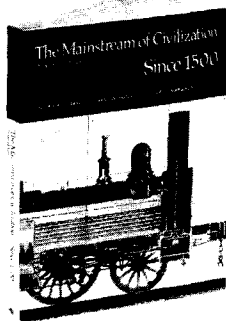


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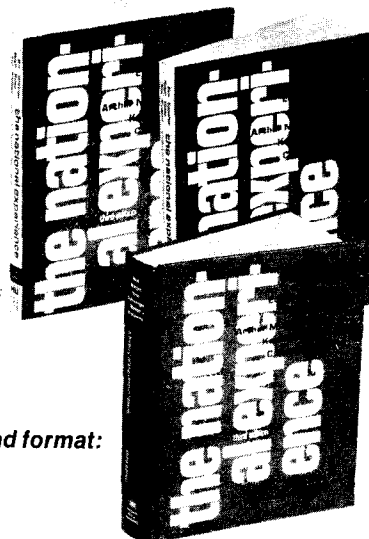
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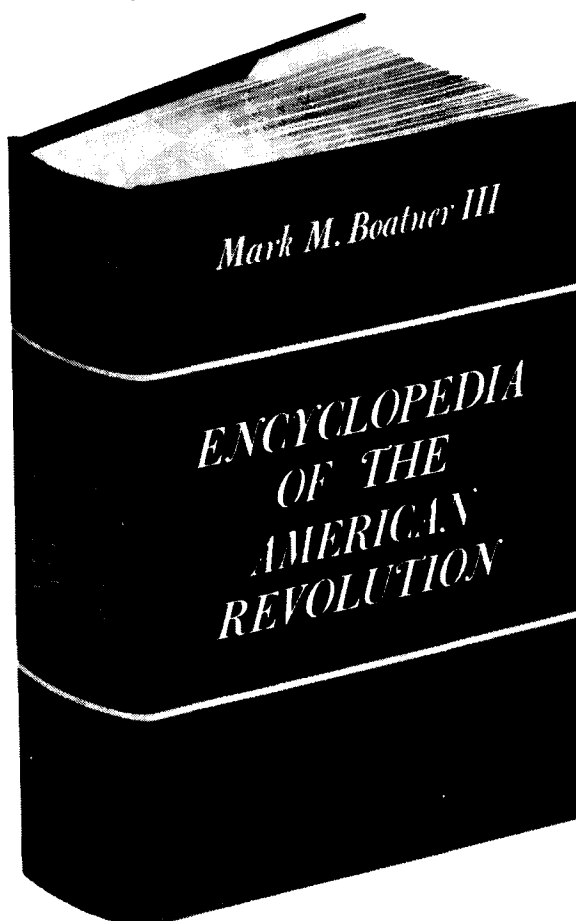
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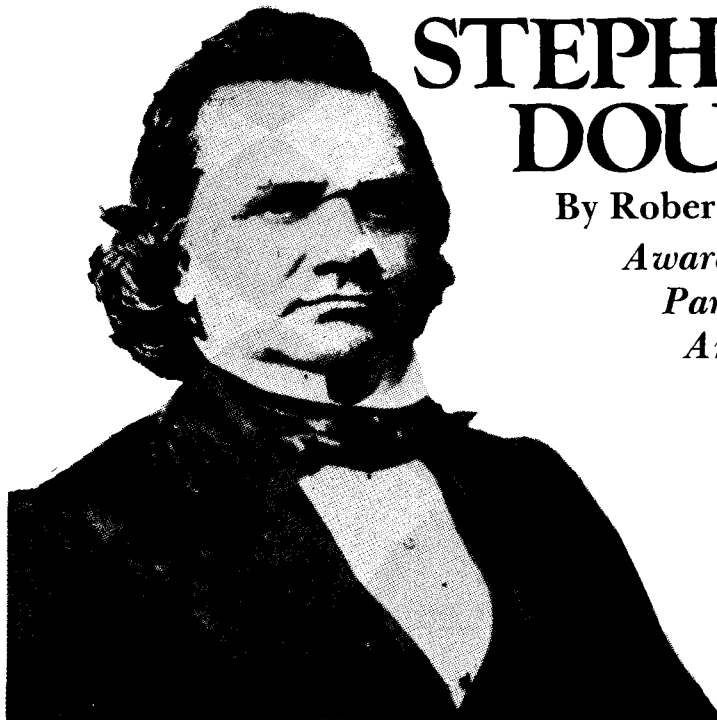
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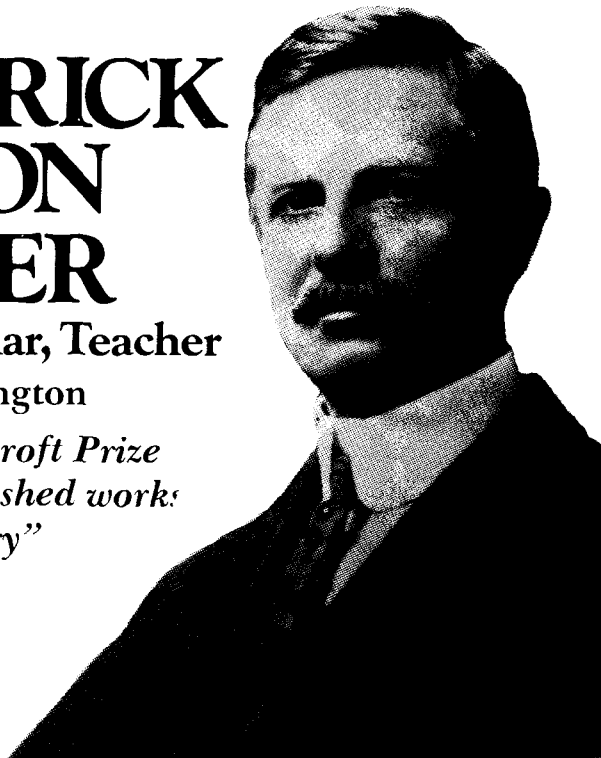
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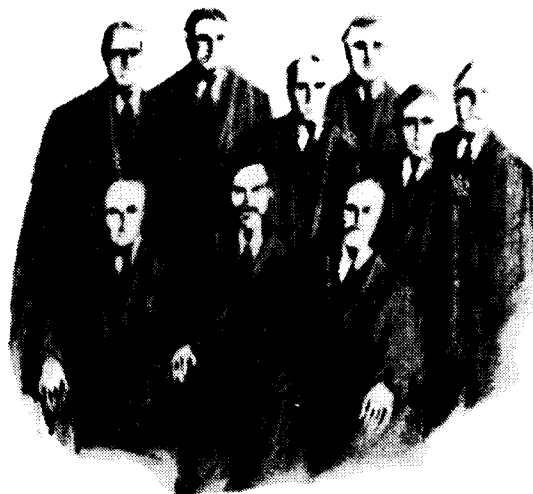
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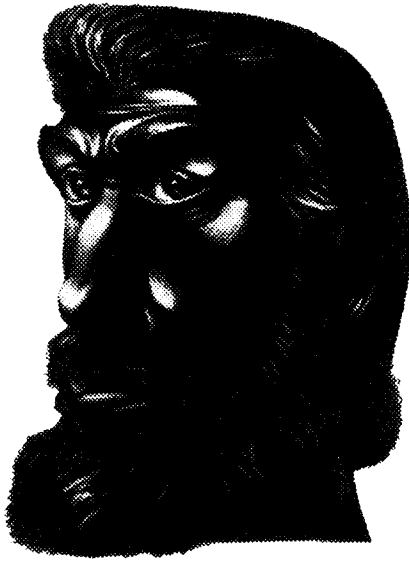
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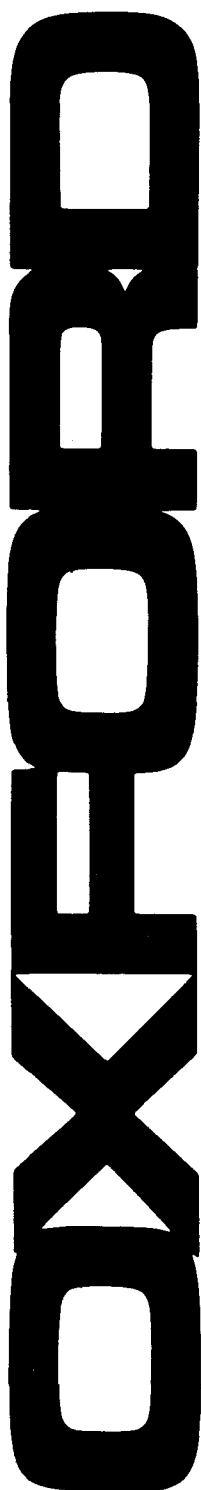
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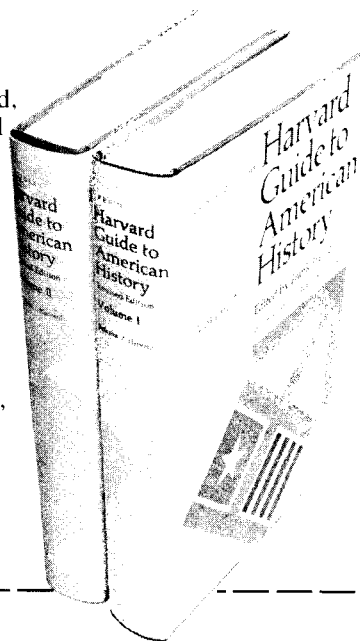
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By RICHARD J. BONNIE and CHARLES H. WHITEBREAD II. xiv, 368 pp., illus., bibliog., index.

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